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SOME PROBLEMS OF SCIENTIFIC THINKING*

By Sir Frederic Bartlett

St. John's College, Cambridge

In all forms of skill, including thinking, the successful performer has to learn how to get ready for things before they actually happen. For the scientist this generally means that he must exercise fairly long-distance foresight, or prediction. Two forms of scientific prediction must be distinguished. In the first the discovery of what seem to be fragments of evidence reminds the expert of some structure to which the fragments may belong. He may then say: " if I go on searching, this and that are what I shall expect to find, and very likely all the details will eventually fit together into such and such a structure ". This sort of prediction has been of great value in all branches of morphological science but, as T. H. Huxley long ago pointed out, it explains nothing, but is rather a prospective, or retrospective, classification. Some scientific thinking tries to give what is essentially the same sort of thing an experimental form, saying: " Assuming that our principles (or assumptions) are correct, then, if we arrange experiments in this way, these are the results we may expect to find ". In fact it seems as if unfulfilled predictions of this kind are more useful than fulfilled ones. Far more scientifically interesting is that kind of foresight which, very often a long time ahead, indicates those directions and lines of research which are the most likely to be fruitful. For this the scientist must be able to select, or perhaps to construct experimentally, the appropriate and significant evidence. It seems also that he must be in some special manner sensitive both to those directions of exploration which are moving towards a desired issue, and, negatively, to the nearness of obstacles. There have been many suggestions that more and more, as time goes on, scientific thinking is best carried out in groups or teams. A recent American movement, called ' Brainstorming ', is said to have some bearing on this claim, but it is doubtful in what sense this is the case.

§ 1. Anticipation and Prediction

Thinking can be profitably considered as an advanced, or high-level, achievement for which preparation has already been made in the course of the establishment of the varied forms of bodily skill. Anybody who cares to look closely at the expert performance of bodily skill, can hardly fail to notice its anticipatory character. If a motion picture is made of any sequence of skilled behaviour, performed by animal or man, and then its exposure is stopped at some point short of the terminus of the sequence, it can readily be seen that the performer is as much oriented towards what is just coming as he is determined by what has just been left behind. His balance of postures, very often the direction of his regard, what pattern of muscular response is being formed, and what sensory receptor processes are being facilitated—all these show that the expert skilled performer is set to appreciate evidence before the moment for action upon that evidence has actually arrived. How this anticipatory characteristic of bodily skill has been developed is not our concern at present, though much light has been thrown upon the process by recent experimental studies (e.g. Welford 1958, Chapter II). What is immediately to the point is that as more and more advanced forms of skill become possible, this feature of anticipatory response gains vastly increased range, and also new qualities of its own.

* Based on the Huxley Lecture delivered at the University of Birmingham on 13 March 1958.

One of the special forms of anticipation which thus come to be effective is of particular importance for scientific thinking. It is, in the main, problems arising from this that I propose to try to discuss, though I do not know whether I can find fully satisfactory answers.

First, because the verb ' to think ' is used very loosely, not only in everyday conversation, but often enough in what claims to be serious discussion, I must say briefly, but definitely, what I am going to take to be the distinctive requirements of scientific thinking. Such thinking is based upon evidence that is, or has been, selected from a situation, or situations, which are, or were, open to sensory observation. It consists essentially in an expansion of this evidence, without violence, through a closely related sequence of steps, to some point which is treated as a terminus, at least for the time being. The steps are by no means always fully stated before the terminus is reached, but in scientific thinking it is always assumed that there are steps, and that they can be set out in such number and order that absolutely any thinker who accepts the evidence and passes through the step sequence must arrive at the same terminus. Further, there is some place in the sequence beyond which, for every thinker, the subsequent steps will be the same in number and in order. One way and another everybody agrees that scientific thinking has a strong compulsive character. In the view that I have just stated this is to be seen particularly in the nature of the passage through steps to terminus.

What is it that has to be taken as ' evidence ' ? From our immediate point of view it is sufficient to say that ' evidence ' is whatever the thinker collects and selects from his environment and uses to pass through steps to a desired or satisfying terminus. A full discussion of the influences which guide the collection and selection of environmental detail, and the manner in which, and processes through which, they work, would take us far away from our main topic. Certainly they come into play long before it becomes possible to ' think ' in the sense in which I am using this word. They operate, for example, at every level of perception, even the most rudimentary (Bartlett 1954, Chapter II). It is also true however, especially in a great amount of scientific thinking, where long continued streams of connected research may be required, that the collection and sifting of evidence may itself require precisely the step-wise progress of thinking. There is in fact no circle involved if we say both that thinking is that set of activities which treat evidence in the way just described, and also that evidence very often demands thought if it is to be effectively collected and sifted.

We may now return to the point at which I broke off just now. The anticipatory character of bodily skill is obviously tied up with the immediate environment in which the animal or human being is operating: but the evidence upon which scientific thinking is based may come from whatever may be either perceived or remembered. Largely to mark this difference what is called anticipation in skill, is very often called ' prediction ' or ' foresight ' in scientific investigation.

§ 2. Prediction in relation to Structure

It is fitting to take the basis and functions of prediction as introducing the problems of scientific thinking which I am now to consider, for in various ways these matters particularly interested Huxley himself. Let me say at once that

there are at least two sorts of prediction, each much used in scientific thinking, which ought to be carefully differentiated.

First there is the kind familiar in morphological science, or wherever there is accredited evidence of the way in which items and details of phenomena are built into a definite structure. Then, if some items only and their relations have been found, it may be predicted that further research will discover other items, and perhaps their specific relations. Huxley (1881) himself said quite a lot about this in one of his most entertaining lectures. Perhaps his own most interesting example was the identification by Cuvier of certain fossilized teeth and a lower jaw as belonging to the opossum family, and Cuvier's prediction that additional exploration of the site from which these remains had come, would reveal so-called ' marsupial bones '. It did, and Cuvier's comment was " This operation was carried out in the presence of a number of people to whom I had already forecast its result, my intention being to prove to them the correctness of my zoological theories. The most convincing mark of a true theory is, beyond any doubt, the power which this theory confers to foresee phenomena."

Huxley was clearly impressed by this sort of thing, but he was not carried away by it, and he made it clear that such prediction cannot help the scientist in his search for causal relations between phenomena. At best all it can do is to show that a certain body of incomplete evidence has been rightly, or wrongly, classified.

Prediction of this kind however can be used, and often is, where there is no question of labelling a particular class of structure, but rather one of deciding what must happen when certain experiments are performed. The procedure is to assume, with or without admission, a knowledge of the principles involved, then to fix upon some instance in which the principles are operative, and to ' predict ' what will happen if that instance is experimentally realized. No harm is done except in the instances in which, when the ' prediction ' is found to come true, this is treated as a proof of the validity of the principles*. It is, of course, nothing of the kind, and in this sense it is all too easy to exaggerate the positive function of prediction in experimental science. Unconfirmed predictions of this kind are probably of very much greater interest and value than confirmed ones.

Apart from this specific kind of case, predictions which look forward to the production of anything genuinely new are very much less common than is often supposed. The authors of a recently published book called *The Sources of Invention* say that " experience suggests that the most *specific* inventions were not foreseen ", and they record abundant evidence of this. " No one, least of all the inventors concerned, predicted the discovery of penicillin, nylon, polyethylene, the transistor, insulin, radio, the cyclotron, the zip fastener, the first

* The procedure is possibly more common in biological than in physical science, and it may be most common of all in experimental psychology. On a fairly recent visit to U.S.A., I attended many public discussions, and a large number of these were concerned with theories based upon some aspect or another of the alleged relation between central nervous functioning and the way in which various electronic devices work. Very often the argument went like this. " If we feed into an electronic network this sort of input, these are the results, or output, that we shall get. Let us then see if we can use this network as a ' model ' and feed the same class of input into the central nervous system. If the ' model ' is correct we shall get the same kind of output." So far as I can remember the experimenter always did get what he expected. It is not only in America that this procedure is used.

aniline dye, the vulcanization of rubber, or many other cases which could be quoted." (Jewkes *et al.* 1958.)

§ 3. Prediction and the Development of Method

There is a second kind of prediction which is much more scientifically important and interesting, and how this is achieved is far less easy to understand. Sometimes this is distinguished from specific prediction and called ' prophecy ', perhaps because it concerns methods rather than results. It is well illustrated by Huxley's own prophecy, made in the early 1880's, that one of the most profitable lines for medical science to study would be the experimental investigation of the production of disease by minute infective agencies, and the parallel development of more delicate and exact techniques to reveal the nature of these agencies. Considering the time at which this was made, and the accredited evidence that was available then, this was a brilliant bit of foresight and everybody now knows how right he was.

A recent, and more concentrated instance is Sir Wilfred Le Gros Clark's (1955–6) forecast that the investigation of the spatial and temporal patterns of response set up by the stimulation of nerve endings is by far the most likely line along which the probable physical basis of sensory discrimination in the skin will come to be understood. Indeed I believe it is not too much to say that all effective scientific thinking anticipates the directions in which it is going to be most worth while to look for advance in whatever field of knowledge is concerned, and in many instances also—more and more in fact as time goes on—the methods which it is going to be most fruitful to use.

It is often said that because it is rare that specific new results of research are predicted, we must agree that they are often stumbled upon by accident. Except in the merely tautologous sense that ' accidental ' means nothing more than ' not specifically foreseen ' there is little or no justification for such a view. No scientific investigator who uses his capacity to think is willing, or able to explore along all the directions that are theoretically possible. Indeed it is unlikely that any, even teams of investigators, ever have explored or could explore all the lines that are practically possible.

How then can we understand the mental processes and mechanisms by which the scientific thinker comes to be able to foresee what lines and methods of experimental investigation, or what dimensions of experimentally controlled change, are going to be worthwhile following up ?

Sometimes—and probably more often in scientific thinking than in any other kind—such forecasts are clearly founded directly upon a wide and critically considered array of factual evidence. Le Gros Clark, for instance, arrives at his final suggestion through a brief, but comprehensive, survey of a great bulk of earlier work on the specificity of nerve fibres, leading to a more concentrated study of the free nerve endings of the cornea, and to the apparent breakdown of simple notions of specific nerve energy in the very region where there seemed the greatest chance of their being confirmed. But it does not seem that the sort of scientific forecasting with which I am concerned can ever be fully understood in terms of an examination of all the lines of search or dimensions of change that have been tried, and their exclusion on grounds of incomplete success. For one thing, it never does happen that only one possible line of search, or dimension of change remains open. For another, there are

plenty of cases like Huxley's, where available scientific factual studies are too sketchy, few and incomplete for any such method of exclusion to be available. For yet another there are those instances in which sheer lack of information about the relevant experimental studies is nevertheless associated with outstanding capacity to select beforehand fruitful lines and methods for study, and the most appropriate variables for control. Joseph Priestly, for example, writing about his *Observations on Different Kinds of Air* said: " When I began these experiments I knew very little of chemistry and had, in a manner, no idea on the subject ..... But I have often thought that upon the whole this circumstance was no disadvantage to me; as, in this situation I was led to devise an apparatus and process of my own ..." (quoted by Huxley 1881, p. 98).

§ 4. Detecting the Direction of Evidence

This kind of scientific forecasting is a particular case of the general characteristic of successful thinking, that it is able to pick out the direction in which evidence is proceeding and willing to take a risk and plan experiments on what is thus selected. Its own special interest—for the psychologist at least—is that the direction has to be appreciated by immediate factual observation, aided in most instances by appropriately selected recall. So far as I know nobody has as yet fully succeeded in understanding how the potential directions of evidence can be perceived, or appreciated in some other way. It seems that the process must be based upon the essential step-wise arrangement of thinking, and it may be that for any terminus there are a number and order of steps that are the most appropriate, whether the terminus can be stated before it is reached or not. Perhaps therefore in scientific thinking the all-important response to direction depends upon three things:

1. The selection, or experimental construction, of the appropriate empirical evidence.
2. Superior sensitivity to what may be called a viable step sequence.
3. Superior sensitivity to the nearness of obstacles when some particular step has been taken, or is about to be taken.

As regards the selection, or experimental construction, of appropriate empirical evidence:

I suppose that nearly everybody who has discussed the place of hypothesis in scientific thinking has quoted Darwin's assertion: " Without hypothesis there can be no useful observation ". Huxley was of course well aware of this statement, and himself quoted it, but he put the proper place for hypothesis later in the course of research, when, the required empirical data having been collected, the scientist sets out to demonstrate causal relations between them (e.g. Huxley 1894). Darwin's form is, in fact, a variety of the kind of prediction that I have just been considering at some length. He says in effect: " If we examine this selected region of phenomena, but without further bias or prejudice, we shall most likely be able to assemble the facts, or experimentally to produce the evidence, from which we can start to establish the inevitable interconnections which we seek ".

If Huxley's hypotheses are to be regarded as predictions at all, they are nearer the first sense that I considered. They say: " Assuming the relations

between this and that group of phenomena to be so and so, if we set up this experiment we shall find that these particular instances of the group of phenomena will behave in such and such a manner which we can specify beforehand. If they do, our assumption is correct, and to the degree to which they do not do it is wrong." I believe it will be found that in all really effective use of hypothesis of this style the scientific thinker is willing to consider, and often actually does formally consider, at least two hypotheses if not more.

Perhaps it is hardly necessary to point out again that whatever type of hypothesis we are concerned with, if it is genuinely successful, in the great number of cases, indeed perhaps in all, it comes from outside the immediate field that is concerned. This is true whether the science is fully mathematical or chiefly experimental. A large part of modern experimental medicine owes its advance to suggestions and speculations that come from physics and electronics. It is, of course, too early to say, but according to the reports, if Heisenberg successfully achieves a unified field theory, it will be through his use of the new experimental ideas about parity which come from the work of the two young Chinese physicists who were given a Nobel prize in 1957.

§ 5. Scientific Sensitivity

As regards the superior sensitivities which scientific thinking seems to require, when any experimental scientific research is near the beginning there are almost always a large number of theoretically possible early steps. Some of these will not be considered, because they are not empirically possible, and, so far as I have been able to determine experimentally, not all of the empirically possible ones will be considered either. But after some of the earlier steps have been taken, the number of empirically possible further steps may be found to be progressively diminishing, and this may be how a viable step sequence is defined. Eventually the successful scientist finds himself in a position something like that of the air pilot coming in to land on a beam. The pilot has extremely sensitive instruments which will tell him whether he is keeping on the beam path. I cannot see how we can get away from admitting that at least some outstandingly successful original scientific thinkers possess a superior sensitivity, within themselves, to those steps next to be taken in a sequence which will preserve a line of progressive reduction of empirically probable further moves. What are the source and nature of this superior sensitivity I do not know.

There is a third feature too, which also seems to require a superior sensitivity, this time to potential obstacles and dead-ends of investigation. Blind people make use of sound echoes to direct their path of movement which remain totally unnoticed by sighted people. It seems that the most effective scientific thinkers may have some special sensitivity which quickly warns them of the neighbourhood of difficult obstacles. Then they avoid the path upon which they have set out altogether, or, having taken it, they turn away and try a different approach more quickly than other explorers.

It is a curious thing how often the scientist who is working hard at some problem to which, as yet, he does not know the complete answer, likes to have, at the same time, some subsidiary, and apparently unrelated problem, to which he can turn with relief from time to time. It seems, in fact, very likely indeed that those mental activities which are the most sensitive to whatever it may be that lies ahead of the stages already reached in specific investigations are

often most effective when it may look as if they are being given a rest. Various possible explanations can be suggested, but nobody yet knows which is the most important, or whether all of them play a part. Maybe close preoccupation with immediate evidence hinders anticipatory thinking, and then the alternative problem may work simply by removing some temporary block. Or perhaps, sometimes at least, this is a special case of the fact that the most valuable hints for good lines of investigation come from fields outside the one immediately concerned. Or yet again the main use of the alternative problem may be related to the observations made long ago by William James when he said " We learn to skate in summer and to swim in winter ". At any rate it is certain that a great many scientists develop the habit of working at more than one problem, or at more than one aspect of the same general problem, over the same period. And I do not think that this habit is confined to the scientific thinker either. It appears to be common among people who are attempting constructive thinking in any field.

§ 6. Invention, ' Brainstorming ' and Social Influences

I now propose to consider what will at first seem to be a very different theme. Is it possible to estimate the degree to which scientific thinking may have become a socialized activity ?

Invention is not, of course, quite the same as thinking, and many a first-rate thinker has never, particularly in a practical sense, invented anything. Nevertheless most, or even all, inventions involve some thinking, and there is no scientific invention without scientific thinking. Early in *The Sources of Invention*, the book to which I have already referred, a number of opinions are quoted, several of them American, all to the effect that modern scientific invention is a social affair. I was, then, exceedingly interested to receive a report of a full-scale investigation, carried out at Yale University, and entitled *Does Group Participation when using Brainstorming Facilitate or Inhibit Creative Thinking* ? (Taylor *et al.* 1957.) I have to confess that when I received this report I did not know what constitutes ' brainstorming ', though I did know that, in general, if popularity is sought for a person, or a theory, or a book, or a movement, it is a good thing to give it a nickname. Brainstorming, it turned out, is a method devised by Alex F. Osborn, a member of an advertising agency, and it has apparently achieved a lot of popularity in America, and is used in colleges and universities, industrial training concerns, and in the fighting and civil services. The plan is to present a problem and to require a person, or people, to produce about it as many ideas as possible. It is assumed that the larger the number and the more varied are these ideas, the greater is the chance of reaching a solution of the problem. It will be apparent that this ' throwing out ' of ideas is not by itself what I should call thinking, since that for me always means putting the ideas into a sequence which moves with increasing certainty towards a terminus. In brainstorming there are four basic principles to be observed. They are:

1. Criticism is to be ruled out. Adverse judgement of ideas must be withheld till later.
2. The wilder the idea the better.
3. The more ideas the better.
4. Joining different ideas together and combining them into something new is to be encouraged.

In its most widely accepted form, brainstorming is carried out by group collaboration, and the Yale experiments were designed to show whether " group participation when using brainstorming facilitates or inhibits creative thinking ".

I cannot stay to describe the experiments or their results in detail. Within the assumptions made they seem to me to have been well designed and executed, and a statistical analysis of the results was carried out with skill and good judgement. The general upshot was that the group products were inferior to those of an equal number of individuals, working not as a group, in *number*, *originality*, and, according to the methods of assessment used, in most cases in *quality*.

A second experimental study has since appeared, entitled *Should Group or Individual Work Come First on Problems Requiring Creative Thinking When Equal Time is Devoted to Each* ? (Taylor and Block 1957.) The general upshot was much the same as that of the earlier study. It is stated that " more ideas are produced by a given number of individuals working alone than by the same individuals working in a group whether individual work comes before or after group work ". Putting the two discussions together it seems to be strongly implied that group participation hinders effective (' creative ' is the word actually used) thinking. It is, however, pointed out that the results must not be generalized beyond the conditions of the experiments; in particular the groups employed were small and the time allowed for group discussion relatively short.

These experiments, in fact, illustrate very well a point that was made earlier. If I am right, it is correct and useful to define scientific thinking as a special way of dealing with evidence. But this does not mean that the evidence which is dealt with is immediately open to anybody, and is merely, so to speak, waiting to be perceived. Especially when scientific research on any topic has already proceeded for some time, there is almost always a considerable number of experimental variables that might be made a subject of further study. Some of them, however they may be controlled, are not very likely to produce anything positive or definitive about the topic under investigation. To choose or decide upon those that are likely to reach the required terminal stages itself requires a process of scientific thinking, which thus determines, first the sort of evidence that is to be considered, and secondly how to design and conduct the experiments which will expand this evidence into the form and order leading to an issue that can be set out in a compelling manner. One of the besetting troubles of experimental scientific thinking is to take far less precautions than are necessary in the selection of variables for study. In this case, those picked out are: number of ideas, peculiarity, or ' wildness ' of ideas, and freedom from criticism of ideas.

As William Harvey, Darwin, and Huxley, and many another first-rate scientific thinker have all declared and demonstrated, there can be no effective experimental scientific thinking until the material for it has been prepared by impartial and informed observation. In an age of great specialisation it may be well the case that working in teams is essential for scientific advance, and that whether or no teams are necessary, scientific thinking is becoming more and more a socialized function. But if this is so it is at least likely that this is because these are the surest ways of submitting ideas to the most rigorous criticism, and of working them into that kind of ordered step

relationship which leads to an issue that all thinkers must accept. It seems then that the problem of the extent and nature of the social contribution—if there is any—to scientific thinking still remains wide open for further and different experiment.

Dans toutes les formes d'activité, y compris la penseé, il faut, pour réussir, apprendre à se tenir prêt aux évènements avant qu'ils ne se produisent en fait.

Pour le scientifique, ceci signifie généralement qu'il doit développer des facultés de prévision à longue portée ou de prédiction.

Il faut distinguer deux formes de prédiction scientifique.

Dans la première, la découverte de données fragmentaires rappelle au spécialiste la structure de laquelle ces fragments pourraient provenir.

Il se dira alors: " si je poursuis mes recherches, je puis m'attendre à trouver ceci ou cela, et il est très probable que tous les détails vont se fondre dans telle ou telle structure ".

Cette sorte de prédiction a joué un grand rôle dans toutes les branches de la science morphologique, mais comme T. H. Huxley l'a fait remarquer il y a longtemps, elle ne constitue en rien une explication, mais plutôt une classification avant terme, en même temps que rétrospective.

Une certaine pensée scientifique essaie de donner une forme expérimentale à ce qui est essentiellement la même démarche de l'esprit, disant: " Admettant que nos principes (ou suppositions) soient justes, si nous combinons des expériences de telle ou telle façon, nous pouvons nous attendre à trouver tels résultats ". En fait, tout se passe comme si la non-vérification de telles prédictions était plus utile que leur vérification.

De beaucoup plus intéressant au point de vue scientifique est ce genre de prévision qui, très longtemps à l'avance, pressent les directions et les lignes de recherche les plus vraisemblablement fructueuses. Pour faire cela, le scientifique doit réussir à choisir les données appropriées et significatives, voire à provoquer expérimentalement leur apparition. Il faut aussi qu'il soit doué en quelque sorte d'un sens spécial l'avertissant à la fois des directions de recherche qui mènent à une issue souhaitée, et, au contraire, de la proximité des obstacles. On a souvent suggéré que, de plus en plus, avec le temps, la pensée scientifique se réalise au mieux en groupe ou en équipe. On dit qu'on mouvement américain récent, appelé ' Brainstorming ' confirmerait cette vue, mais il subsiste des doutes quant au niveau de cette pensée en équipe.

Bei allen Formen der Arbeit, die hohe Geschicklichkeit erfordern, einschliesslich der Denkarbeit, muss der erfolgreiche Arbeiter lernen, auf Geschehnisse vorbereitet zu sein, bevor sie wirklich eintreffen. Für den Wissenschaftler bedeutet das ganz allgemein, dass er ziemlich weitreichende Voraussicht oder Voraussage üben muss. Zwei Formen wissenschaftlicher Voraussage müssen unterschieden werden. Bei der ersten Form erinnert die Entdeckung von scheinbaren Fragmenten den Fachmann an Strukturen, zu denen diese Fragmente gehören könnten. Er sagt dann vielleicht: " wenn ich weiterforsche, erwarte ich das und das zu finden und sehr wahrscheinlich werden alle Einzelbefunde möglicherweise in der und der Struktur zusammenpassen ". Diese Art der Voraussage war in allen Zweigen der morphologischen Wissenschaft von grossem Wert, erklärt aber, wie T. H. Huxley vor langem ausführte, garnichts, sondern ist eher eine vorausblickende oder rückblickende Klassifizierung. Manches wissenschaftliche Denken versucht, dem eine experimentelle Form zu geben, was imgrunde das gleiche ist. Es sagt: " Angenommen, dass unsere Prinzipien (oder Voraussetzungen) richtig sind, dann sind die und die Ergebnisse zu erwarten, wenn wir Experimente auf die und die Art anorden ". Tatsächlich scheint es, als ob die sich nicht erfüllenden Voraussagen dieser Art besser zu gebrauchen sind als erfüllte. Wissenschaftlich weit interessanter ist die Art Voraussicht, welche oft langfristig voraus in jene Richtung und zu jenen Grundlinien weist, die am wahrscheinlichsten fruchtbar sind. Dazu muss der Wissenschaftler befähigt sein, die passenden und signifikanten Beweise auszuwählen, oder vielleicht sogar experimentell zu konstruieren. Es scheint auch, dass er in einer besonderen Art für jene beiden Forschungsrichtungen empfänglich sein muss, welche sich auf das gewünschte Ergebnis zubewegen und negativ empfänglich für nahende Hindernisse. Es sind viele Vermutungen laut geworden, dass in Zukunft wissenschaftliches Denken am besten mehr und mehr von Gruppen oder Teams durchgeführt wird. Eine neuere amerikanische Bewegung, ' Brainstorming ' genannt, soll etwas mit dieser Forderung zu tun haben. Es ist jedoch zweifelhaft, in welchem Sinn das der Fall ist.

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ACCIDENT-PRONENESS IN MOTOR-VEHICLE DRIVERS

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Motor-vehicle drivers were examined at interview and on visual tests. Each was classified as ' accident-prone ' or ' safe ' on the basis of his known accident record and the distance he said he had driven, those with a rate of 3 or more accidents per 100 000 miles being classified as ' accident-prone ', those with a lower rate as ' safe '. On this criterion there were 34 ' accident-prone ' drivers to be compared with 74 ' safe '.

All those who gave a history of definite nervous or mental illness were ' safe '. Those who had had grammar-school education or its equivalent tended to be ' safe '. The unmarried men tended to be ' accident-prone '. A larger proportion of the ' accident-prone ' had been convicted of traffic or other offences. No differences were found between the ' accident-prone ' and the ' safe ' in the incidence of physical disabilities, eye or ear defects or psychosomatic complaints, or in personal qualities so far as these could be rated. Plans for further research into accident-proneness are briefly discussed.

§ 1. Introduction

The investigation to be described consisted of the psychological and medical study of a group of motor-vehicle drivers whose accident records suggested that they were accident-prone and their comparison with a group of drivers with good records. It was expected on the basis of reports in the literature (e.g. McFarland *et al.* 1955, Häkkinen 1958) that the accident-prone drivers would show personal qualities or medical disabilities not shown, or shown in lesser degree, by the safe drivers.

Our own results do not fulfil this expectation, although they should perhaps be taken with caution, because of deficiencies in our methods of sampling. The investigation was intended as a preliminary to a more thorough and extensive one, but the experience gained in carrying it out led us to make a drastic revision of our plans, for reasons which are discussed later.

§ 2. Method of the Enquiry

Selection of the Subjects

We started on the investigation with the intention of selecting suitable groups of ' accident-prone ' and ' safe ' drivers from files kept by Cambridge City police. These recorded under the driver's name every road accident occurring within the City of Cambridge and reported to the police, whether involving ' personal injury ' or ' damage only ', from 1946 up to the present time. From these files we picked out at random two groups of drivers: those who had sustained three or more accidents and those who had sustained one accident. We then wrote to each person so selected, explaining briefly the purpose of our investigation and what was required of him. Several days later a visit was paid to his home, and an appointment made for his attendance in the laboratory.

One hundred and twenty persons were written to. Of these, 28 had moved from the address given in the records and could not be reached or were otherwise unavailable. Of the remaining 92 persons, we succeeded in examining 59;

* In receipt of a personal grant from the M.R.C.

33 either refused outright or made difficulties about attending in the laboratory or failed to keep one or more appointment. The distribution of the cases between the two groups is shown in Table 1.

Table 1. Selection of Cases from Police Records

	One-accident	Three or more accidents	Total
Selected	59	61	120
Not reached	15	13	28
Refused	16	17	33
Examined	28	31	59

After we had carried out a number of examinations of persons so selected, we became dissatisfied with the one-accident group as representatives of 'safe' drivers. The names of drivers who had sustained no accidents did not appear in the police files. We enquired into the possibilities of obtaining the names and addresses of licence holders from the licensing authorities, but it proved impracticable to do so.

We partially solved the problem of finding accident-free drivers by enlisting the help of those firms whose drivers had competed in the Safe-Driving-Award scheme. All the firms we approached gave us ready help in providing subjects. We checked in each case the admitted accident record against the police records and found that although these subjects were in the main picked out because of their safe-driving record for the firm, and their availability, and could therefore be regarded as suitable representatives of safe drivers, the names of several of them appeared in the one, two or even three-accident lists in the police records. This group thus gave a spread, ranging upwards from entirely accident-free. We examined 67 subjects obtained in this way from five firms.

Altogether we examined 126 persons, 59 selected from the police records, and 67 obtained through their employers. These persons differed very widely in their driving experience. We decided, therefore, to re-group them according to the type of driving in which they had mainly been engaged, although this made it impossible to compare each 'accident-prone' driver with others of similar experience.

The 126 subjects fell conveniently into seven groups according to the class of vehicle driven, as shown in Table 2.

All those in groups (1), (2) and (3) had been selected from the police records. Some of those in group (4), which included those using a car daily in the course of their work, e.g. taxidrivers, police patrol drivers, commercial travellers and supervising officers from the Post Office had been selected from the police records, and some had been sent by their employers. In this re-grouping of our subjects we excluded 18 cases: all 5 women, because their total mileage was very low, and their driving experience was very different from that of the men; all 6 public-service-vehicle drivers, because their duties were too diverse for them to be compared with one another; and 7 others, who could not readily be classified, or about whom information was incomplete.

In order then to classify each subject as accident-prone or safe, we had to decide on a suitable criterion. The number of accidents by itself did not seem to be appropriate. For instance, the man who in the course of 10 years had driven 20 000 miles and sustained three accidents was obviously not to be compared directly with a man having had the same number of accidents but having driven 250 000 miles. Scrutiny of our data suggested that the best criterion would be the number of accidents per 100 000 miles, and we therefore classified the subjects thus:

' Safe '—less than three accidents per 100 000 miles;

' accident-prone '—three or more accidents per 100 000 miles.

Table 2. Classification of Subjects

	Number of subjects	Number of accidents	Accident-rate per 100 000 miles	Safe	Accident-prone
(1) Private cars	11	22	3·8	5	6
(2) Light goods vans	12	36	1·6	8	4
(3) Heavy goods vehicles	15	47	1·6	9	6
(4) ' Professional ' cars	28	56	1·2	23	5
(5) Delivery trucks (British Railways)	8	23	2·8	4	4
(6) P.O. Engineering vans	18	31	2·0	13	5
(7) Tankers (Shell-Mex, B.P. and Esso)	16	22	0·6	12	4
	108			74	34

For this purpose we used the subjects' own estimates of his mileage for each year of the past 10 years and summed them. We took account of all the accidents known to us, i.e. those appearing in the police records, those in the records of the firms and those admitted by the subject. The application of this criterion led to the final arrangement of the subjects shown in Table 2, and gave us 74 safe subjects and 34 accident-prone.

Examination of the subjects

We depended mainly upon interviewing as a means of obtaining information. An interview of about half an hour usually proved sufficient to cover all the relevant points, although it sometimes took much longer when subjects talked freely about themselves, as many did. A few were reticent to begin with, but then opened up. In a few cases direct questioning proved necessary, but

usually subjects offered most of the required information in the course of conversation, which was guided by the interviewer. The interview tended to follow a standard pattern, and at its conclusion certain information had been obtained from each subject.

Driving experience. How he had learnt to drive; training courses; mileage each year; type of driving; cars owned; jobs as driver; and accidents, traffic offences and convictions.

Personal history. Schooling; job record; war-service experiences and jobs; domestic situation; housing; leisure activities and habits; interests.

Medical. Visual, auditory or other physical defects; illnesses; hospital admissions of subject and family; ailments, especially skin conditions, dyspepsia, rheumatism and headaches.

Attitudes. Views on traffic problems, roads and accidents.

After the interview was completed, visual tests were made. Acuity was assessed for each eye separately, using a Snellen Test-type, viewed at a distance of six metres. We also made tests of the rotating-block and fan types for astigmatic defects, and tested the binocular vision field on a perimeter.

§ 3. Results

Table 2 shows that there are differences in accident rate between the various groups. The ' private cars ' have the highest rate, 3·8 per 100 000 miles, and the ' tankers ' the lowest, 0·6; the rate, 1·2, in ' professional cars ' is relatively low. These differences make sense, but it is necessary again to warn that comparisons between the groups are not strictly legitimate, because they were selected quite differently, and because for some groups there were more sources of information about accidents than for others.

Age

No relation between age and accident record was found.

Marital status

There were 9 single men in the whole sample. Seven were in the accident-prone group, two in the safe. Of the two single men in the safe group, one had suffered from a relatively severe pulmonary tuberculosis, involving chest operations, and the other suffered from a distinct, although not incapacitating, mental disorder.

The interpretation of the finding that the unmarried men tended to have a poor accident record ($P < 0{\cdot}05$) is uncertain. As a group they were no younger than the married men. It is tempting to argue that they had remained unmarried because of defects in personality which also play a part in accident-proneness.

Education

Twenty subjects in the whole sample had had grammar-school education or its equivalent, all of them in the ' police ' sample. Of these 18 were in the safe group, two in the accident-prone as shown in Table 3. It appears that education in this sense reduces accident-proneness.

Training courses

No differences between accident-prone and safe were found in the number or type of courses of instruction in driving.

Table 3. Accident Record and Education

(Police sample only)

	Number of subjects	Grammar school or equivalent	Others
Safe	41	18	23
Accident-prone	18	2	16

The association between ' safe ' driving and grammar-school or equivalent education is significant ($P < 0{\cdot}05$).

Medical

The most striking finding is that all of the eight subjects in the whole sample who had suffered from a definite nervous or mental illness were in the safe group. Minor nervous symptoms were equally common in the two groups.

Most of the illnesses reported were characterized by anxiety and depressive symptoms, and occurred in persons who were hard-working, conscientious, cautious or timid when they were well. It seems probable that these are favourable qualities in drivers. The subjects did not drive during the acute illness, and we were not told of any accident which could be attributed to nervous or mental illness.

A few of the subjects had suffered from serious physical illnesses or defects, but no relation between these and accident records was found. Minor medical disabilities were equally common amongst the safe and the accident-prone. There was one markedly deaf person in each group; those with slight defects in hearing were also distributed evenly between the two groups.

We found no reason to attribute any of the accidents of which we were told to a physical disability or defect in hearing or vision. It appeared that those who so suffered had compensated well. However, we studied the relation between visual defects and accident-record in several ways, with different criteria, but without significant finding. Table 4 shows that there was no more than a slight, insignificant tendency for those with reduced visual acuity, i.e. poorer than 6/9, to be in the accident-prone group.

Table 4. Accident Record and Visual Acuity

	Number of subjects	Good acuity	Reduced
Safe	74	61	13
Accident-prone	34	24	10

The difference between the two groups is not significant.

The occurrence of psychosomatic affections, such as asthma, skin diseases, rheumatism, dyspepsia and headaches, was scrutinized, and the data were arranged in various ways, using various criteria and classifications. Table 5 shows that the trend was for the incidence in the safe group to be higher, although not significantly so, when present, but not past, complaints were considered.

Table 5. Accident Record and Psychosomatic Complaints

	Number of subjects	Number with complaints	Total number of complaints	Severe in past	Severe in present
Safe	74	62	119	23	34
Accident-prone	34	29	50	15	11

None of the differences between the two groups is significant.

Personality

Various ratings were made of anxiety, aggressive tendencies, psychopathic and antisocial tendencies and similar qualities. No relation between these ratings and accident record was found. There were a few subjects who were found to have marked aggressive, psychopathic or antisocial tendencies, but there was no preponderance of such subjects amongst the accident-prone.

Convictions

We obtained information about the police-court convictions of each of our subjects. The results are set out in Table 6.

Table 6. Accident Record and Convictions

	Number of subjects	Number with convictions	Number traffic convictions	Number of convictions for other offences
Safe	74	20	34	2
Accident-prone	34	17	20	4

The proportion with convictions is significantly larger among the accident-prone than among the safe ($P < 0{\cdot}05$).

A larger proportion of the accident-prone than of the safe had been convicted of offences, either traffic or other offences. When the actual number of offences was considered, the correlation with accident record proved not to be significant. There appeared to be no consistent difference in the type of offences committed by the two groups, but the data were insufficient for a firm conclusion to be drawn. Those who had been convicted did not in general show any distinctive characteristics and could not be regarded as of psychopathic or abnormal personality.

Other factors

A number of other factors were considered, such as job record, housing and domestic stresses, but no positive findings were made.

The attempt was also made to relate particular accidents to changes in the subject's domestic situation or emotional state, but no consistent trends could be demonstrated.

§ 4. Discussion

The records kept by Cambridge City police promised to have considerable value for an investigation into accident-proneness, but, although they could be used to provide samples of subjects with a bad accident record, they could not provide comparable samples with an accident-free record who could act as control subjects. Moreover, the drivers whose names appeared in the police records were widely heterogeneous and had to be classified arbitrarily before worth-while comparisons could be made. As our investigation proceeded, we came to the view that more useful samples could be obtained from large firms employing relatively large numbers of drivers on similar duties. Private drivers are for a variety of reasons the most difficult of all types of subjects to study, and it would seem more profitable in any further investigations to obtain subjects from large firms, until the research problems are more clearly defined.

Our investigation was exploratory, and covered a large number of variables. Any further investigations should be designed with more specific objectives, and with reference to a few variables only. For the study of some of the variables, such as age, marital status, driving experience and education, all that is necessary is the collection of simple facts, interviewing and medical and psychological examination being unnecessary. The required data could be more suitably obtained, therefore, from the records of insurance companies or the police than through any investigation similar to ours, in which the number of subjects is necessarily limited.

For the study of medical factors and especially factors in visual functions, the most promising course now would appear to be the launching of an investigation in one or more large firms, who could provide relatively homogeneous samples.

The practical difficulties of studying the personal qualities which contribute to accident-proneness are great. It might be argued that our failure to demonstrate distinctive personal qualities in the accident-prone was due to the deficiencies in our investigation. Certainly the selection of groups of accident-prone and safe drivers for comparison was beset with many difficulties, which were not fully overcome, and the total number of subjects was relatively small. Moreover, we depended upon the information obtained at interview and did not use any of the standardized personality ' inventories or tests ', for none seemed suitable.

We suspect, however, that a more important reason for our negative results is that our hypotheses were too crude, for we supposed that we would find enduring qualities in the accident-prone. It may well be that accident-proneness is composed of changing and changeable qualities of personality of a kind that would not appear at interviews carried out months or years after the accidents have been sustained. If this view is correct, then investigations like ours will not yield positive results even if deficiencies in methods and selection of subjects are overcome. Positive results are more likely to be obtained if subjects are examined soon after they have sustained accidents, or if studies are made of the accident records of subjects selected because they show disorders of personality of various types.

On a examiné des conducteurs de véhicules motorisés à l'aide d'interviews et de tests visuels. Chacun d'eux a été classé ' prédisposé ' ou ' non prédisposé ' sur la base de la fréquence de ses accidents en fonction de la distance qu'il a dit avoir parcourue. Ceux dont la fréquence des

accidents est égale ou supérieure à 3 pour 100 000 miles parcourus (160 000 km) sont classés ' prédisposés ', ceux dont la fréquence est inférieure à 3 sont classés ' non-prédisposés '. Par ce critère on distingue 34 conducteurs prédisposés et 74 conducteurs non-prédisposés. Tous les conducteurs ayant dans leurs antécédents une maladie nerveuse ou mentale font partie des ' non-prédisposés '. Ceux qui ont suivi l'enseignement secondaire ont tendance à être ' non-prédisposés '. Une plus grande fraction de conducteurs ' prédisposés ' ont été condamnés pour des délits de route ou autres. Mais on n'a trouvé aucune différence entre ' prédisposé ' et ' non-prédisposés ' quant au rôle des infirmités, des défauts de vision ou d'audition, ou des symptômes psychosomatiques, ou encore des traits de personnalité, dans la mesure où ces derniers ont pu être évalués.

L'article s'achève par une brève discussion des plans pour une étude ultérieure de la prédisposition aux accidents.

Kraftfahrer wurden in einem Interview und in Seh-Tests geprüft. Jeder von ihnen wurde als ' Unfäller ' oder ' Unfallsicher ' auf der Grundlage seiner bekannten Unfallkartei und der von ihm angegebenen Fahrkilometer eingestuft. Diejenigen mit einem Verhältnis von mehr als 3 Unfällen pro 160 000 km wurden als ' Unfäller ', diejenigen mit einem niedrigeren Verhältnis als ' Unfallsicher ' eingestuft. Nach diesem Kriterium waren 34 ' Unfäller ' mit 74 ' Unfallsicheren ' zu vergleichen.

Alle Fahrer, welche in ihrem Lebenslauf eindeutig nervöse oder geistige Erkrankungen zeigten, waren ' Unfallsicher '. Diejenigen, welche eine höhere Schulbildung oder Gleichwertiges erhalten hatten, waren auch ' Unfallsicher '. Die unverheirateten Männer zeigten Unfallneigung. Die grössere Anzahl von ' Unfällern ' waren wegen Verkehrs- oder anderer Delikte bestraft worden. Bei Auftreten von psychischen Störungen, Augen- oder Ohrfehlern, oder bei Störungen persönlicher Eigenschaften—soweit man diese erfassen konnte—oder bei psycho-motorischen Beschwerden wurden zwischen den ' Unfällern ' und den ' Unfallsicheren ' keine Unterschiede gefunden. Pläne für weitere Forschung über Unfallgefährdung werden kurz diskutiert.

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A NOTE ON PANEL LAYOUT FOR NUMBERS OF IDENTICAL ITEMS

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The spatial layout which is best for a small number of similar items on a panel may not be best when extended to a large number of items. Some brief tests are reported in which the operator's task was the sequential selection of two items from amongst two groups of twenty-four items. The probable bases of the operator's responses and of the optimum design for such panels are discussed.

During the design of an analogue computer advice was sought by the project team on various aspects of the operational layout. A particular problem was the positioning of 24 potentiometers and 24 associated switches in the minimum panel space consistent with easy location and control by the operator. The simple experiments which proved to be necessary are reported here.

An analogue computer is an assembly of many similar electronic units which can be connected together in different ways. The input and output leads of the amplifiers, integrators, and coefficient-setting potentiometers are all brought out separately to sockets on the ' patch panel '. By making the appropriate interconnections the operator can ' patch up ' the computer to solve problems. In setting up the computer one of the basic tasks, essential to the accurate solution of any problem, is to adjust each coefficient-setting potentiometer accurately to the required position. Unless the panel of potentiometers has a clear and simple layout, there is a risk of setting up the wrong potentiometers or of starting to adjust one which has already been set.

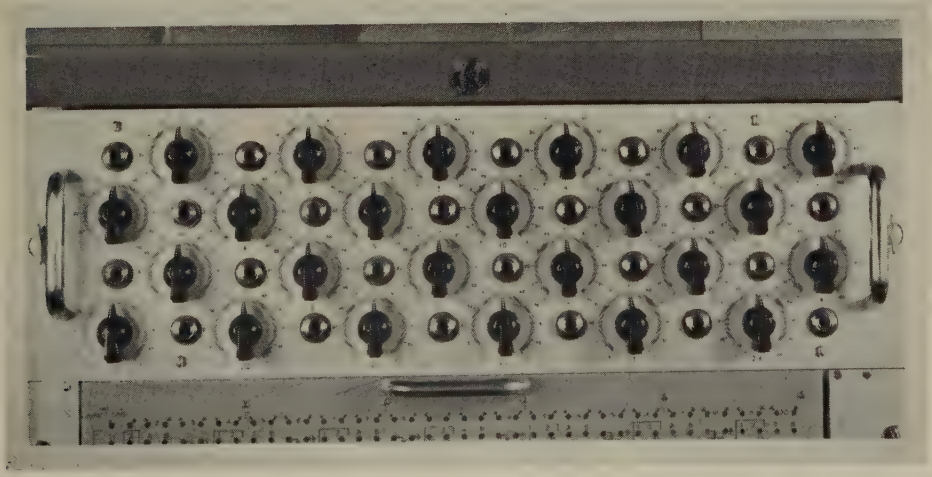


Figure 1. Prototype panel of 24 potentiometers and 24 associated switch-sockets.

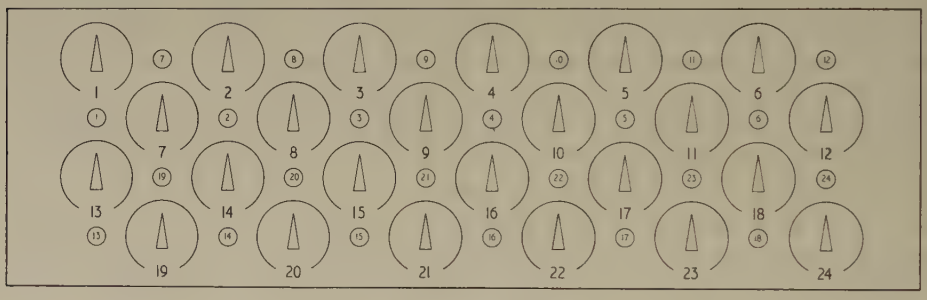
The layout used in the first prototype is shown in Fig. 1. The potentiometer panel is interchangeable and should therefore be as small as reasonably possible. To economize in space the potentiometers were staggered at 45° to one another and the jack sockets, through which electrical connection is made to the potentiometers, were set in between them. The result, however, is

somewhat confusing, and there is no room for numbers to identify each jack socket.

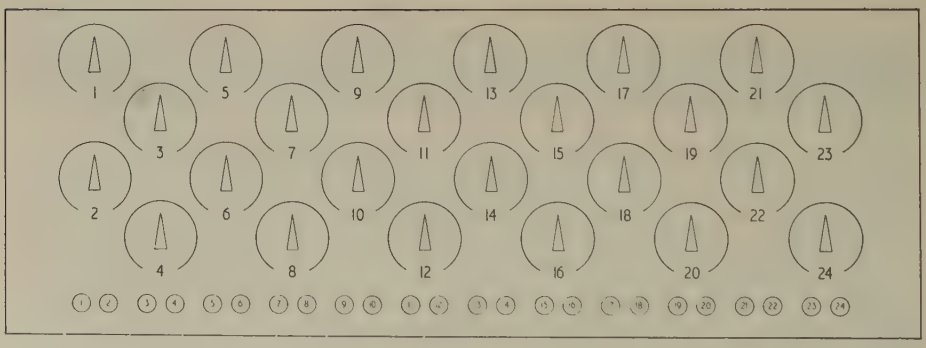
Preliminary discussion yielded agreement on the use of push–pull switches with a number on the knob, to replace the jack-sockets, despite the added electrical complication, and established the possibility of a slight increase in panel height being accepted if necessary. Thus the task was to find a layout requiring the minimum panel size, with which the sequence of operations: (1) find and operate the switch, (2) find the potentiometer, (3) adjust it, (4) turn off the switch—would be done most easily and with the least error.

Test I

The layout of the prototype panel shown in Fig. 1 is sketched in Layout A of Fig. 2. Although basically composed of four simple rows numbered in the customary fashion, the layout is confused by the alternation of the switches above and below their respective potentiometers.



Layout A.



Layout B.

Figure 2.

Because the first part of the sequence of operations involves sequential selection, it is obviously desirable to use the switches as guides to the location of the appropriate potentiometer. This would seem to be done best by simplifying the selection of the right switch and by relating the potentiometers as directly as possible to the switches. A layout designed to achieve this within the space limitations is shown in layout B of Fig. 2; the switches are aligned in one horizontal row, suitably spaced so that from any given switch position the relevant potentiometer can only be one of the two immediately above.

A brief test was made to find whether a real difference in operational performance could be expected between these two panels.

Two layouts were simply constructed on paper by using dye-line copies of the full-scale panel drawings for the prototype. By adding rough circles where necessary, and by numbering all switches and potentiometers, layouts A and B were represented in what was considered to be an adequate fashion.

The subject was seated comfortably in front of a desk; he held the layout flat in front of him with his non-preferred hand, and used his preferred hand to stab the forefinger down as quickly as possibly onto the switch and potentiometer positions appropriate to the number called out by the experimenter. On each trial the subject was asked first to close his eyes; then, as soon as the experimenter called out a number (and started a stopwatch), the subject opened his eyes and tapped the appropriate switch and potentiometer numbers on the layout; the experimenter stopped the stopwatch as soon as both had been tapped.

Two sequences of the numbers 1 to 24 in random order were compiled. Four subjects were used; the order of panel presentation was reversed between the two pairs of subjects; both of the random sequences of numbers were done by each subject on each layout, again in a balanced, reversed order.

As well as recording the time taken, a note was made of any errors and of the subjects' preferences. It should be emphasized that under the normal working conditions of the final equipment speed is not expected of the operator. Therefore the time-scores here taken were not meant to be meaningful in relation to the actual operating conditions, but were used, along with the other measures, as indicators of which layout was found to be easier and simpler.

It can be seen from Table 1 that performance with layout B was superior both as regards speed and accuracy, and that this layout was preferred by all the subjects.

Table 1. Performance with Layout B was superior to that with Layout A

	Layout A	Layout B
Mean response time ($\bar{x}$ of 192 trials on each layout)	2·8 sec	2·0 sec
Total errors	10	1
Preferences		' Easier ' (2 subjects) ' Much easier ' (2 subjects)

Performance on layout B was significantly faster: the difference between the correlated scores of the four subjects was significantly different from zero; $t=6{\cdot}71$, $n=3$, $P=<0{\cdot}01$.

Test II

The layouts shown in Fig. 3 are those recommended by Ely *et al.* (1956) in their general review for equipment designers; the recommendations are based upon one report (Nystrom and Grant 1954) and the expert opinions of Woodson (1954) and themselves. Extrapolation from these recommendations to a layout as similar to B as possible, to meet spatial requirements, yields either layout C or layout D in Fig. 4. These two layouts were therefore tested along with layout B by a method similar to that of Test I.

Six subjects were used. Three separate sequences of the numbers 1 to 24 in random order were compiled. The three number sequences were given to all subjects in the same order, but on each sequence each subject used a different layout. The order of presentation of the three layouts B, C and D was thus rotated among the six subjects, each subject having a different one of the six possible orders. Otherwise, the instructions and method were identical with those of Test I.

The results, set out in Table 2, show performance with layout B to have been faster although not more accurate than with layouts C and D. Again layout B was preferred by all subjects. Three subjects described why B seemed easier, in terms such as " I felt as if I only had one task to do on B, to find the number in the bottom row and then just go up or down above it,

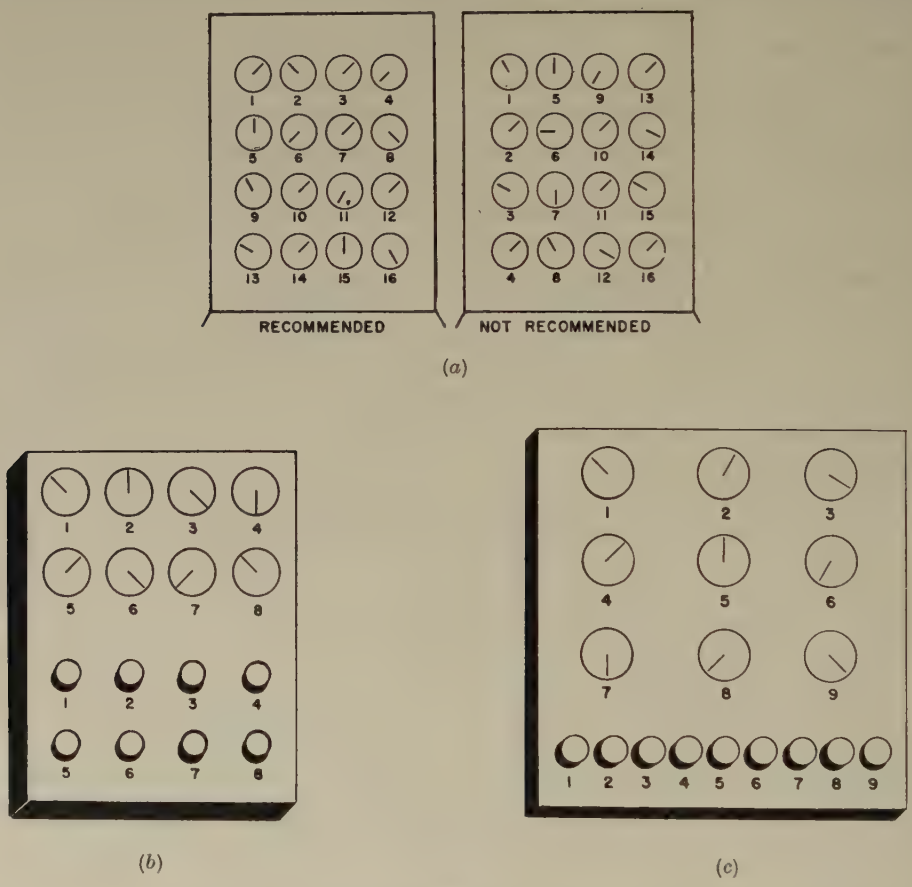
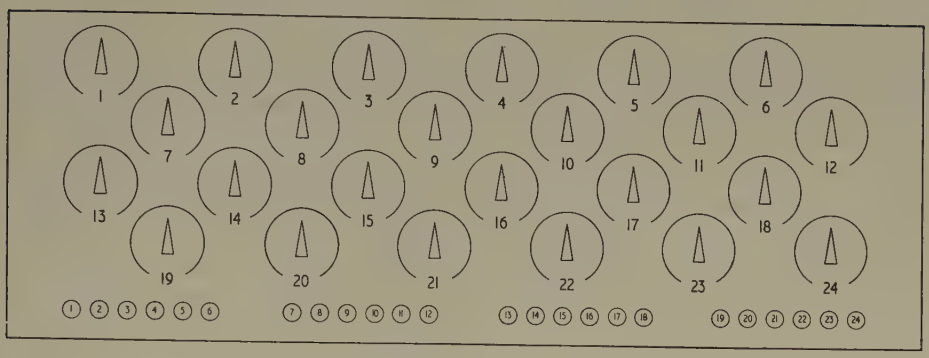


Figure 3. Recommendations from Ely *et al.* (1956).

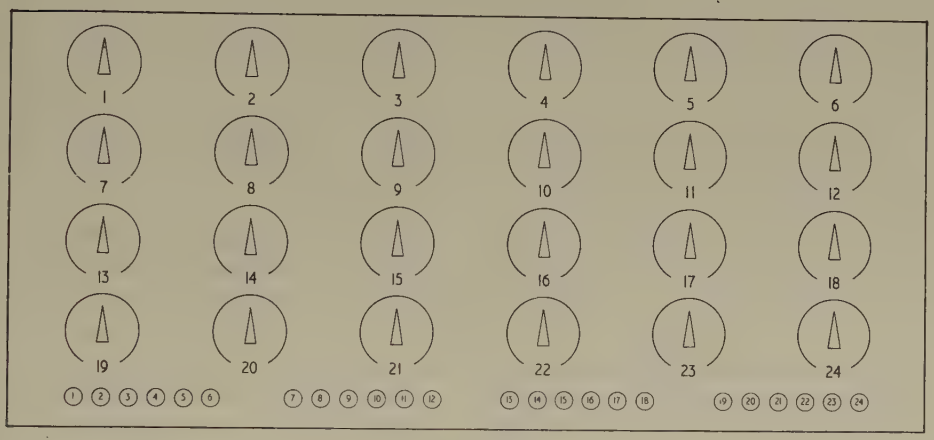
(*a*) " Displays should be aligned so that they are in the following sequence; . . . Rows: from top row to bottom row, and from left to right within a row." (Woodson 1954; expert opinion.)

(*b*) " When a large number of displays are on the same panel, they should normally be arranged in either of the following two ways: (i) each display directly above its associated control . . . (ii) all displays in the upper portion and all controls in the lower portion. Each control occupies the same relative position as its associated display." (Nystrom and Grant 1954; experiment.)

(*c*) " When two or more rows of displays must be associated with one row of controls, and vice versa, the general arrangement shown should be used." (No reference.)



Layout C.



Layout D.

Figure 4.

Table 2. Performance with Layout B was superior to that with either C or D.

	Layout B	Layout C	Layout D
Mean response time ($\bar{x}$ of 144 trials on each layout)	2·0 sec	2·4 sec	2·4 sec
Total errors	4	3	5
Preferences	'Much easier than C or D' (2 subjects) 'Fair bit easier than C or D' (2 subjects) 'A little easier than C or D' (2 subjects)	'A little easier than D' (1 subject) 'No difference' (3 subjects)	'A little easier than C' (2 subjects)

Performance on layout B was significantly faster than on C or D: the differences between the correlated scores of the six subjects were significantly different from zero; D–B, $t=5{\cdot}27$; C–B, $t=6{\cdot}30$; $P=<0{\cdot}01$ in each case.

whereas on the others I had to find a number both times ". One subject said that B definitely seemed a little easier although he felt that C or D ought to have been easier because the horizontally aligned rows of potentiometers were easy to read and 'looked usual'.

Discussion

The subjects were chosen to be representative, in intelligence level and prior training, of the sort of person likely to use analogue computers and similar electronic machines. Although more subjects would be needed to determine a population stereotype, it seems reasonable to conclude that layout B is a real and useful improvement over both the original layout and the type of layout which might well be extrapolated from the presently available advice.

The difference between layouts B and D would seem to depend upon the fact that, while both require the same type of selection among the horizontal row of numbers at the bottom, D subsequently requires a second, comparable, selection process amongst the block of potentiometers, with little or no effective transfer from one to the other in spatial terms. In B, on the other hand, the spatial grouping links the two selection tasks directly and greatly minimizes the selection possibilities for the second task as soon as the first task has been done.

In double selection tasks of this kind the differences between the optimum layouts for a large and a small number of items probably depend upon the type of selection process which is naturally adopted by the operator. When dealing with a small number of items, for instance nine in three rows of three or eight in two rows of four, the spatial layout of the items is probably within the capacity of some kind of ' spatial memory span ' and is therefore learned rapidly and retained well. Moreover, when there is a double operation involved, such as of switch and potentiometer, the jump from one to the other can be made directly in spatial terms. However, when there are more items in the layout than can be retained within the ' spatial memory span ', selection has to be made in terms of the number code.

From these suggestions it would follow that the optimum layout might well be different for different numbers of items. For a small number it would be the one with which the operator most easily learns to translate the number code into an ' immediate ' response; a layout following the common reading pattern (i.e. with numbers increasing in successive rows from left to right and downwards) seems to be best. For a large number of items the best spatial layout seems to be one which combines the common reading pattern for the first selection with an arrangement which makes the second selection follow as uniformly and straightforwardly as possible from the first.

If this is true, the interesting problem remains of determining at what stage of complexity the selection process changes from the simpler to the more elaborate process. Learning of the task during the operator's work will doubtless alter the level of complexity at which this change occurs for any given operational system. The tests reported here, in conjunction with previously reported work, can do no more than suggest that this changeover occurs somewhere between eight or nine items in rows of three or four and twenty-four items in rows of six. Subjectively one has the impression that the change occurs when there are more than about ten to twelve items. However, more experiments would be needed to determine this matter.

Sur un panneau, la meilleure disposition spatiale d'un petit nombre de commandes utilisées en suites identiques d'opérations peut n'être plus aussi avantageuse pour un plus grand nombre de commandes similaires.

L'article décrit quelques essais expérimentaux de dispositions spatiales dans lesquelles l'opérateur doit utiliser successivement deux commandes à choisir dans deux groupes de vingt quatre commandes. Dans la discussion sont envisagées les interprétations rendant vraisemblablement compte des modes de réponse des opérateurs et de la valeur respective des diverses dispositions des panneaux.

Die räumliche Anordnung, welche für eine geringe Zahl ähnlicher Schaltorgane auf einer Schalttafel optimal ist, muss nicht notwendig auch für eine grössere Zahl von Schaltorganen optimal sein. Es wird über einige kurze Testversuche berichtet, in denen der Prüfling aufeinanderfolgend immer 2 Schaltorgane aus 2 Gruppen von 24 zu wählen hatte. Die wahrscheinlichen Grundgesetze des Verhaltens des Prüflings und des optimalen Aufbaues solcher Schalttafeln werden diskutiert.

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THE EFFECTS OF APTITUDE, FITNESS, PHYSICAL WORKING CAPACITY, SKILL AND MOTIVATION ON THE AMOUNT AND QUALITY OF WORK

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Two experiments are discussed. They were designed for the determination of the best work method from the point of view of ergonomics. Methods for the detection of long term effects on the state of health are indicated. The economy of the work has been judged by comparison of the effort expended and the resulting performance. The results of these measurements are also largely influenced by the personal capabilities for the performance of the particular task by each of the subjects. An analysis of this capability is given.

Attention is also given to the difficulties of comparing work methods when the composition or the use made of the teams of subjects varies. If different groups of subjects are compared, each using their own work method, the groups may differ too much in general working capacity; if the same group applies different work methods, experience in using one method will be greater than with another. Adjustment of the energy expenditure to compensate for differences in pace or quality were not considered feasible.

§ 1. Introduction

In Holland today training programmes are organized for all kinds of jobs. It is not uncommon to find that the same industrial task is carried out according to several different methods. Sometimes at the Institute for Preventive Medicine we are asked to advise on the best work method to be taught in such a new training programme. What is then called ' the best method ' depends largely on the points of view and on the interests of those who give the advice. In the consideration of these problems, attention should be given not only to economy of effort and to the quality of work, but also to the mental and physical well-being of the worker and to his future health.

This paper gives a brief account of the approach to the problem adopted in our Institute. It emphasizes the difficulties in the use of data collected from two field experiments—one with dock-workers and one with pavers—and the need for a better understanding between time and motion study engineers and physiologists.

§ 2. Methods of Investigation

Hitherto the economy of different work methods has been studied in our laboratory by the measurement of pulse rate and energy expenditure during the actual work. The subjects were always experienced at their job. The groups compared were almost identical with regard to age and stature and worked according to the different work methods under study; alternatively the same group of subjects worked in different ways in succession. Both methods have advantages and limitations.

The time and motion study engineers have always made their measurements both before and during the physiological experiments. Any impairment of performance due to the physiological measurements or unusual working rate during the experiment could be detected in this way. The work output per unit of time (production rate) and an assessment of the quality of work were also reported upon by the time and motion study engineers, who were well acquainted with the work under study. The energy expenditure and the number of pulse beats occurring during the time required for unit work and the quality of work obtained were the criteria used to determine the most economical work method.

People should, however, be taught the most healthy way of working, which may not necessarily be the most economical way. Bad postures and undesirable movements should certainly be avoided. In the investigation of the pavers, reported in this paper, the help of industrial physicians and medical statisticians was enlisted in order to find out whether different work methods, implying various body postures, were associated with physical damage which could be related to the method. This study is not yet completed, however.

2.1. *Example* 1*: Dock Work*

In the past, dock-workers never received systematic training. Recently, however, training courses were started and the problem arose as to what work methods should be taught. There were, for instance, four methods of handling a packing case in a ship's hold (Fig. 1). In a laboratory situation four experienced dock-workers, chosen by lot, performed a task consisting of the transportation of four cases from a pallet over a certain distance to a place where these had to be stowed away. Each subject employed each of the four methods of handling. Energy expenditure was measured by indirect calorimetry with the aid of the Kofranyi Michaëlis respirometer (Kofranyi and Michaëlis 1941), and pulse-rates by a cardiotachometer, operating from the electrocardiogram, designed by the Department of Medical Physics, T.N.O. Research Council. The measurement of physiological responses extended over two working periods in each of which four cases were transported, a two-minute rest following immediately after each working period.

The results of these measurements are shown in Fig. 2; the results of the metabolic measurements on one of the subjects have been omitted because of poor fitting of the mouthpiece connected to the respirometer. It is seen that the best method is that in which the case is balanced on the corners (number IV). Energy expenditure, number of heart beats and time required are less than with the other methods. This is not surprising since the theoretical mechanical work is less with this way of handling. The mechanical work done was computed from a formula based on the distance through which the centre of gravity was lifted, which is a function of the way of handling and of the dimensions of the case, on its weight and on the number of revolutions needed for transport over unit distance, which is again a function of the way of handling and of the dimensions. Data for mechanical work in Fig. 2 include the actual distance. It can be seen that the mechanical efficiency, calculated from this amount of mechanical work and the energy expenditure, is higher with method IV than with the other methods. This might be due to the more upright working posture of method IV.

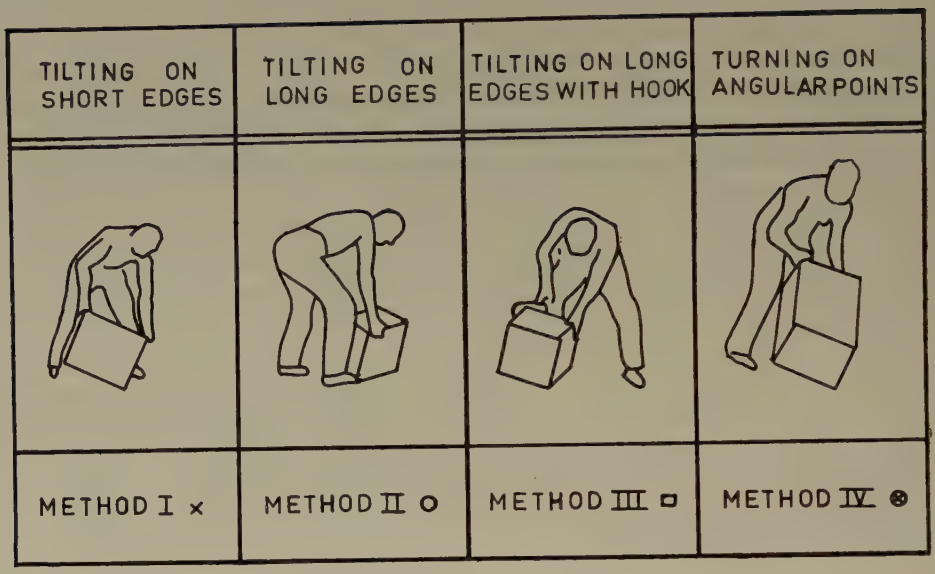


Figure 1 (*a*)

Figure 1 (*b*)

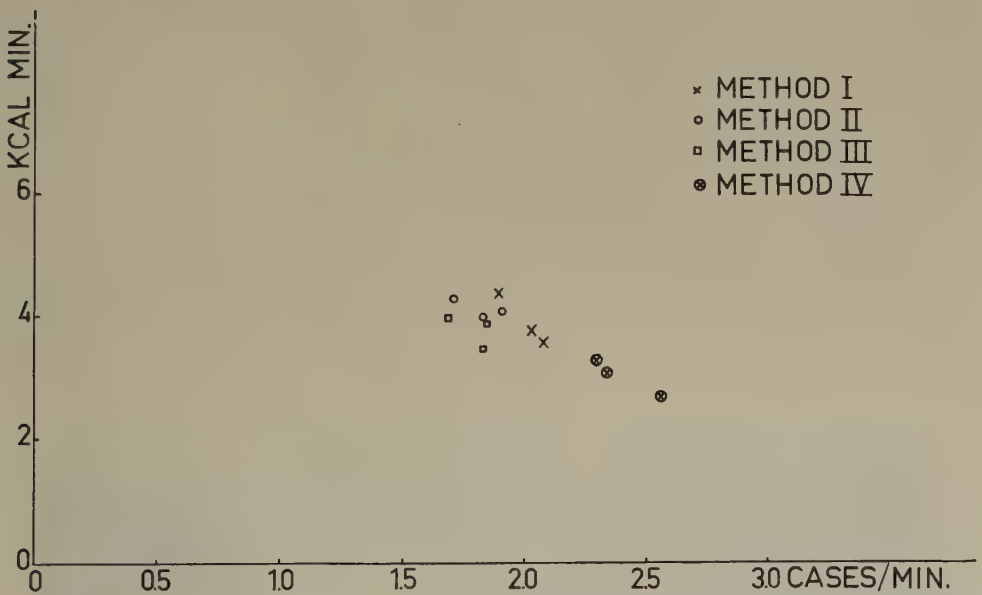
Figure 1. (*a*) Four different methods of handling a packing-case in a ship's hold. (*b*) Method IV applied during the dock-workers' experiment. Note the K.M. respirometer on the subject's back and the cardiotachometer on the table behind the subject.

2.2. *Example 2: Paving Work*

The second example is derived from a study of pavers. There is a geographical distribution in the Netherlands of three work methods employed by pavers, the job being carried out in a standing, sitting or kneeling position. The training committee for pavers wished to have a national training scheme and requested advice on the best single work method, both from the view of

efficiency and of health. How the health problem was tackled has already been indicated.

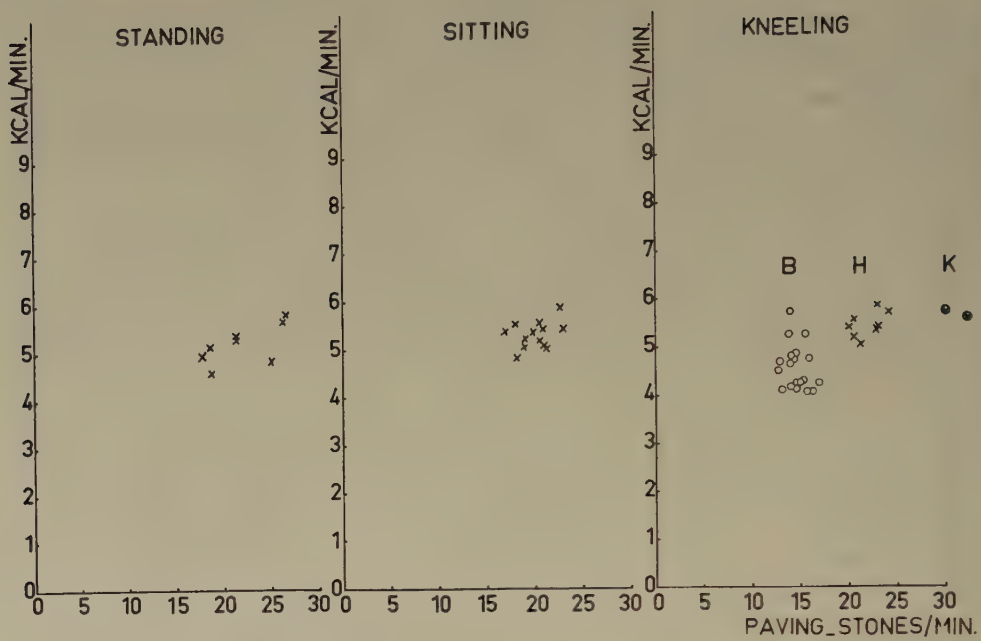
In field experiments the economy of working in the three groups of pavers was studied, each group being experienced in their own style of work. The techniques used were much the same as in the first example, but an I.M.P. (Integrating Motor Pneumotachograph, Wolff 1958) was used. A direct comparison between the three groups proved impossible because of differences in production rate and quality between the groups. The kneeling group, B, spent more time on additional activities and reached a higher quality at low speed. For this reason we studied a fourth group, H, working in a kneeling position but at a production rate closer to the standing and sitting groups.



	METH.I	METH.II	METH.III	METH.IV
TOTAL OF KCAL/CASE	2.21	2.49	2.17	1.17
TOTAL OF HEARTBEATS/CASE	135	136	152	123
MECHANICAL WORK IN KGM/CASE	111.3	107.6	107.6	71.4
GROSS EFFICIENCY IN %	11.8	10.1	11.6	14.3

Figure 2. Dock-workers' experiment: diagram showing energy expenditure per minute plotted against production rate. The table at the bottom shows the energy expenditure, the number of heart beats and the mechanical work required for the transportation of one case. The gross mechanical efficiency has been computed for each of the four methods.

The results are given in Fig. 3. Production rates are given as the numbers of stones laid per minute. The original group B is seen to be different from group H in this respect. Comparison of the second kneeling group H with the standing and sitting groups shows, however, that there is no evidence in these experiments of differences in metabolism related to working posture. Pulse frequencies suggest a preference for the kneeling posture. Special attention is drawn to the high efficiency of subject K in the last group, who came from a family of pavers with a long tradition for this occupation.



VALUES PER 95 PAVING_STONES:

TOTAL OF KCAL.	22.9	25.7	29.6	23.0	13.1
TOTAL OF HEARTBEATS	508	563	701	464	372
			B	H	SUBJ.K

Figure 3 (*a*)

Figure 3 (*b*)

Figure 3. (*a*) Pavers' experiment: diagram showing energy expenditure per minute plotted against production rate. The table at the bottom shows the energy expenditure and the number of heart beats required for 95 paving stones (about 1 m²). (*b*) Standing posture in the pavers' experiment, showing the I.M.P. and the cable transmitting the electrocardiogram.

§ 3. Discussion

It will be clear that there are many difficulties in the approach to the problem of determining the preferred work method. The same subjects may apply different work methods in succession, but usually they are not equally experienced in every method (dock-workers' experiment). When we take several groups, each of which is well experienced in its own method, comparisons are influenced by differences in aptitude, fitness, physical working capacity, skill and motivation. Not only the subjects, but also their performances will change from one group to the other (pavers' experiment). To complicate the situation further the assessment of performance and quality is much more the province of time and motion study engineers, who are acquainted with the type of work, than of physiologists.

In order to assess whether this approach to the problem is adequate, it seems worthwhile to attempt an analysis of the terms used in the preceding paragraph:

Constitutional aptitude is more important for some jobs than for others. Subject K, a paver mentioned before, apparently took the advantage of his origin.

Physical Fitness in the conception used here is the present condition of the body, especially the neuro-circulatory system, as measured by tests according to Flack (1919), Schneider (1920), Crampton (1913) and others. These tests generally involve the measurement of the responses of the body to sudden stresses such as change of posture, of intrathoracic pressure and so on. They are influenced by the present state of well-being and by the physical environment. The physical fitness may change from day to day with the quality of the night's rest, the weather and so on.

Physical working capacity, however, is the product of physical training. It is the capacity of the lungs to transfer oxygen from the air into the blood, the capacity of the heart to circulate the blood and the capacity of the muscles to do work. Physical working capacity can be measured, for example, by a bicycle ergometer test (Wahlund 1948, Åstrand 1958) or a treadmill test (Åstrand 1952) and, much more than physical fitness mentioned above, can be promoted by a physical training scheme.

Manual skill, as affected by instruction and experience, together with constitutional aptitude, physical fitness and physical working capacity determine "the personal capability for a given task". From this point of view no two craftsmen are identical. As a practical measure for this personal capability we may use the quotient :

$$\frac{\text{amount of work}}{\text{total energy expenditure}},$$

or, if preferred:

$$\frac{\text{production rate per minute}}{\text{energy expenditure per minute}}.$$

The total energy expenditure can be expressed in kilocalories or as the total heart beats, the expenditure per minute in kcal/min or pulse rate/min.

The amount of external mechanical work can be expressed in kilogram-metres (kgm). This is readily possible in the laboratory with the aid of a bicycle

ergometer or an inclined treadmill. In industry, however, the mechanical work done is seldom measurable directly. Units of work such as one square metre of pavement, the transportation of one case over a given distance or the assembly of a dozen ballpoint pens are preferable.

A high value of the quotient means that energy expenditure per unit production will be lower, and hence the better is the personal capability for the task. Although the external mechanical work can seldom be expressed in kgm, the quotient is essentially the same as the mechanical efficiency, well known from work physiology.

§ 4. Conclusion

In the evaluation of work methods described in this paper, the relationship studied was one between production rate and caloric expenditure or pulse rate per minute. We may say that, in fact, we measure at least to some extent the personal capabilities for a given task in a group of men. In other words, the judgment about a particular work method is largely influenced by the selection of subjects. This influence should be avoided and taken account of in a well-designed experiment. There is therefore a need for a thorough knowledge of all the various capabilities of the subject taking part. Testing both the physical fitness and the physical working capacity of the subject by known techniques and on the day of the experiments is a step in this direction.

A second conclusion might be that, when work methods are compared in this way, production rates should not differ by too much, since we know that the mechanical efficiency achieved may differ from one production rate to the other. The same applies to differences in the energy expenditure per minute. Time and motion study engineers sometimes take these into account by effort rating. They call effort ' the evidence of the will to work as manifested by a workman performing an operation '. Psychologists may see effort as a direct result of motivation. Experiments of the type described may succeed only if all the subjects are equally motivated. Changes in quality should be excluded as much as possible, as there is no valid method of correction available for extremely high or low quality work, not even when the error is measurable. We found this in an experiment on Hollerith-card punching, in which we studied the effects of different production rates, both on the body and on the quality of work.

L'article ci-dessus contient une description critique de deux expériences concernant la détermination des méthodes de travail optimales du point de vue ergonomique. L'auteur indique les voies permettant de tirer des implications pour la santé et discute l'économie physiologique des méthodes de travail différentes. Celle-ci est jugée par la comparaison de l'effort et de l'effet.

Cette relation étant influencée par l'habileté personnelle des sujets, les facteurs se rapportant à l'habileté sont ici analysés.

Les limitations dûes à la composition et l'emploi des groupes de sujets sont données par:

(1) les différences de capacité de travail générale entre les groupes, si chaque groupe applique sa propre méthode et

(2) les différences d'expérience relatives aux diverses méthodes de travail, dans le cas d'une groupe unique de sujets.

L'auteur est d'avis, qu'il n'est pas permis d'éliminer des variations de rapidité ou d'exactitude en calculant le métabolisme par unité de travail, afin d'obtenir une comparaison plus facile des méthodes de travail différentes.

Zwei Versuchsreihen werden diskutiert, die zur Bestimmung der besten Arbeitsmethode unter ergonomischen Gesichtspunkten unternommen wurden. Methoden werden angegeben, die erlauben, langfristige Wirkungen auf den Gesundheitszustand zu erkennen. Die Oekonomie der Arbeit wurde durch einen Vergleich der aufgewendeten Anstrengung zur vollbrachten Leistung beurteilt. Die Resultate der Messungen werden auch durch die persönliche Geschicklichkeit der Versuchspersonen bei der Ausführung der jeweiligen Aufgabe stark beeinflusst. Diese persönlichen Fähigkeiten werden analysiert.

Beim Vergleich verschiedener Arbeitsmethoden entstehen Schwierigkeiten durch die verschiedene Zusammensetzung der untersuchten Versuchspersonen-Gruppen. Wenn 2 Gruppen von Versuchspersonen jeweils ihre eigene Arbeitsmethode benutzen, kann es sein, dass die allgemeine Arbeitsfähigkeit der Gruppen zu verschieden ist. Vergleicht man dagegen verschiedene Arbeitsmethoden an der gleichen Gruppe, so besteht die Gefahr, dass die Gruppe auf eine Methode besser einge-arbeitet ist als auf die andere.

Verfasser hält es nicht für zu-lässig, beim Vergleich verschiedener Arbeitsmethoden die Aenderungen der Geschwindigkeit oder der Genauigkeit dadurch zu kompensieren, dass er den Stoffwechsel auf die Einheit der Arbeit bezieht.

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CIRCULATORY RESPONSES TO CHANGE FROM RECUMBENT TO ERECT POSTURE AS AN INDEX OF HEAT STRESS

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Experiments are described in which Crampton Index values, derived from measurements of blood pressure and pulse rate following on change in posture, have been related to conditions of environmental heat and to the thermal sensations of resting and working subjects. Experimental evidence is given which indicates that changes in the Crampton Index may be of value in defining individual reactions to increasing environmental heat.

§ 1. Introduction

Many attempts have been made to determine reliable tests for assessing the physical fitness of individuals. Indices of fitness based upon somatometric, physiological and psychological criteria have been reviewed by Burns (1940), Taylor, C. L. (1945) and by Morehouse and Miller (1948). The present paper deals with circulatory reactions (blood pressure and pulse rate) to postural change before and during exposure to environmental heat and humidity, and with the use of these in assessing thermal stress. Leonard Hill (1895) first suggested that reactions to a change of posture might be used to test and gauge the physical condition of an individual. He showed that when physically fatigued subjects changed their posture from a recumbent to an erect position there was a marked fall in systolic blood pressure and an increase in the heart rate but that in physically fit subjects there was a slight rise in the blood pressure. Using changes in systolic blood pressure and pulse rate associated with such changes in posture, Crampton (1913, 1915, 1920) devised a test for assessing individual physical fitness. From observations on several hundred fit young men he found that the changes in systolic blood pressure ranged from $+10$ to -10 mm Hg, disregarding extreme values; in respect of pulse rate there was an increase of 0 to 44 beats per minute. From his analysis of the data, Crampton felt it was justifiable to attach equal significance to a decrease of 1 mm Hg systolic blood pressure and an increase in the heart rate of 2 beats per minute. On the assumption that the observed ranges in these reactions were of equal significance, he constructed a table of physical fitness indices which took into account changes in blood pressure and pulse rate over a very wide range. The ' Crampton Index ' (C.I.) ascribed to various observed changes in blood pressure and pulse rate has been given in the form of a table. (See Crampton 1913.) From the data given in this table the following equation for the evaluation of the Crampton Index has been determined.

$$\text{C.I.} = 2{\cdot}5\Delta x - (5/4)\Delta y + 77{\cdot}5$$

where Δx = observed change in systolic blood pressure mm Hg,

Δy = observed change in pulse rate.

The Crampton Index was first used for assessing the physiological effects of changes in the thermal environment by the New York State Commission on Ventilation (1923). They found that there was a marked fall in the Crampton Index associated with increasing environmental temperature and humidity.

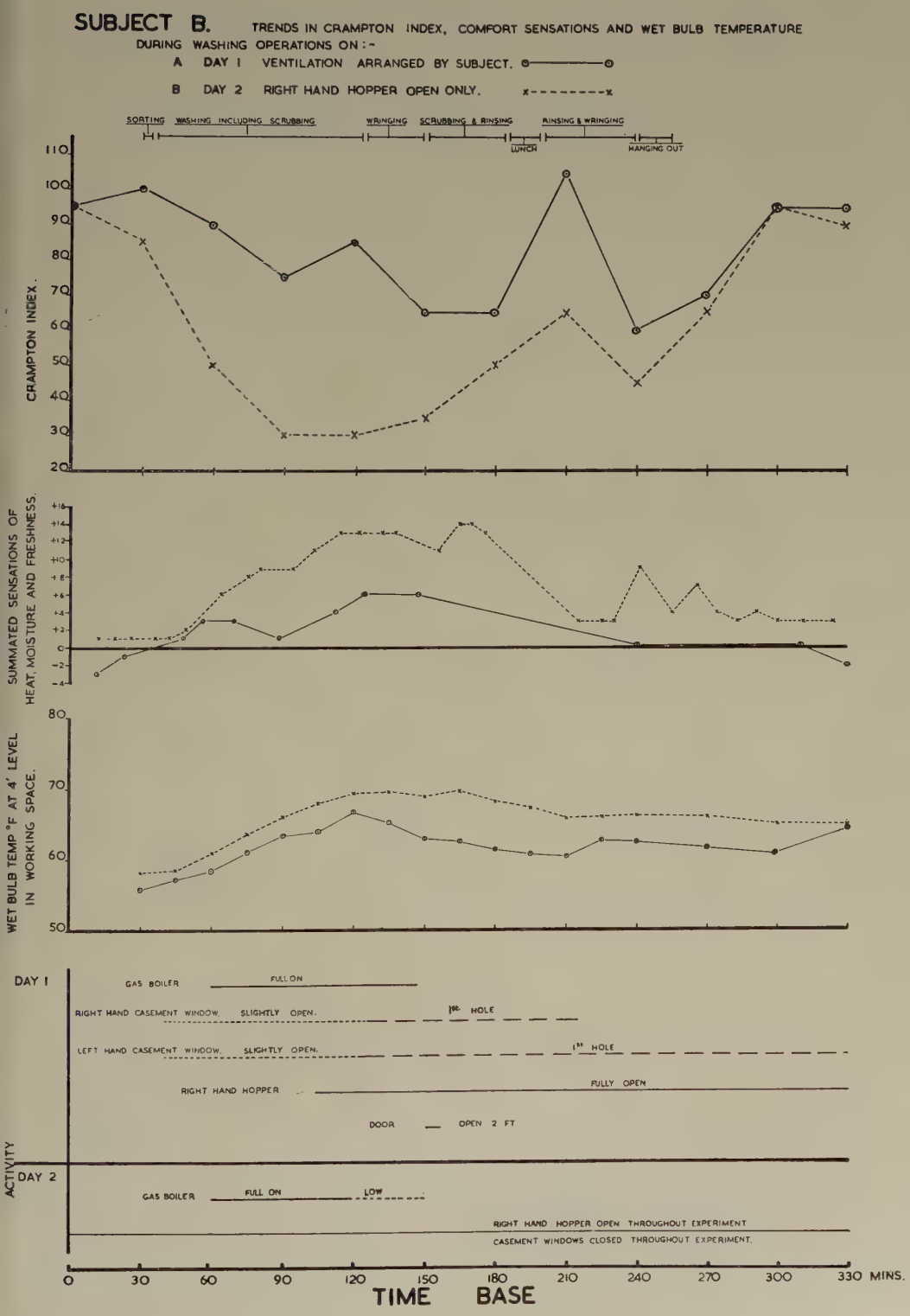

Figure 1. Changes in the Crampton Index of circulatory efficiency and thermal sensations of housewife subject B in relation to variations in the wet bulb temperature during clothes washing.

The Crampton Index has also been used to assess the circulatory efficiency of individuals as judged by absolute values. Scott (1921) found that, over the normal range, which Crampton considered to be between 50 and 100, such individual assessment did not yield results consistent with other criteria of physical fitness and endurance. Crampton held that variations in the index for the same individual were of greater significance than the differences between the indices obtained from a number of subjects. In a series of experiments (Taylor, P. F., 1952, Brown 1955, 1958) leading up to the present study, changes in the Crampton Index were observed in women during experiments on the effects of heat and humidity arising in rooms during domestic clothes-washing operations. An example of these results is given in Fig. 1. The experiments now to be described were carried out to test the validity of the deduction that changes in the Crampton Index were associated with symptoms of heat stress and fatigue and with changes in sensations of thermal discomfort observed in female subjects during domestic clothes-washing. In the assessment of thermal comfort and heat stress the arbitrary numerical scale of thermal sensations shown in Table 1 was used. This scale was derived from those used by Bedford (1936) and Crowden and Lee (1940).

Table 1. Thermal Comfort Sensation Scales

Heat		Moisture	
Sensation	Index	Sensation	Index
Unbearably hot	+7	Unbearably moist	+7
Much too hot	+6	Much too moist	+6
Too hot	+5	Too moist	+5
Hot	+4	Moist	+4
Too warm	+3	Too humid	+3
Warm	+2	Humid	+2
Comfortably warm	+1	Comfortably humid	+1
Neutral	0	Neutral	0
Comfortably cool	−1	Comfortably dry	−1
Cool	−2	Dry	−2
Too cool	−3	Too dry	−3
Cold	−4	Parched	−4
Too cold	−5	Too parched	−5
Much too cold	−6	Much too parched	−6
Unbearably cold	−7	Unbearably parched	−7

§ 2. Experimental Method

The experiments were carried out in an air-conditioned room which measured 12 ft by 9 ft 6 in. by 7 ft in which the wet and the dry bulb temperatures were maintained at the desired levels. An electric fan situated in one corner of the room provided air movement. Six subjects (medical students, 2 female and 4 male) took part, each subject participating in four separate experiments. Two subjects were present in the climatic chamber at a time, one resting and the other performing muscular work (step climbing). The experimental and environmental conditions simulated the build up of heat and humidity during domestic washing operations and were as follows:

(1) Rest and exercise when the dry bulb temperature was gradually increased from between 55°F and 60°F up to approximately 100°F, and

wet bulb from 48°F to 87°F, during a period of four and a half hours, the relative humidity being maintained at 60 per cent.

(2) Rest and exercise with the dry bulb temperature range as in (1) and a wet bulb temperature ranging from 52°F to 94°F, the relative humidity being maintained at 80 per cent.

The clothing worn by each subject was the same for all experiments, viz. a cotton singlet, linen shorts, socks and plimsolls, with the exception of subject A who wore a light woollen singlet and skirt, stockings and light shoes.

During the preliminary stage of each experiment the subjects rested on a couch in the experimental room for half an hour. The dry bulb temperature and humidity were kept constant during this period, at the end of which the Crampton Index was determined and the subject's thermal sensations were recorded.

This preliminary stage was essential to allow the subjects to attain a state of equilibrium. Subsequent reactions to environmental thermal change were measured as deviations from this equilibrium level. The Crampton Index values for the resting and working subjects were recorded at intervals of 16 minutes and their thermal sensations at intervals of 8 minutes throughout the experimental period.

For the evaluation of the Crampton Index the following routine was employed.

The systolic blood pressure and the pulse rate were each measured repeatedly until two consecutive observations were the same :

(*a*) while the subject was at rest in a recumbent position on a couch,

(*b*) after rising and standing for 2 minutes.

The muscular work performed consisted of step-climbing continuously for 2 minutes at intervals of 16 minutes. The step consisted of a 1-ft rise with an intermediate step at the 6-in. level; the subject ascended and descended 30 times during the 2-minute period. During the rest interval, after determination of the Crampton Index, the subject remained recumbent on a couch in the climatic chamber, except during the determination of the Crampton Index.

§ 3. Results

The experimental data for the resting and working subjects have enabled the changes in the Crampton Index values to be correlated with variations in the thermal environment, degree of activity, symptoms of heat stress and sensations of thermal discomfort.

3.1. *Changes in the Crampton Index in Relation to Variations in the Thermal Environment*

Examination of the data for each subject separately gives correlation coefficients between Crampton Index values and wet bulb temperature (in the range from 48·0 to 94·0°F) ranging from −0·619 to −0·969, with a mean value of −0·905, For Corrected Effective Temperature (Bedford 1946), in the range from 54·0 to 94·0°F the correlation coefficients range between −0·737 and −0·990 with a mean value of −0·915 (see Table 2).

Table 2. Correlation Coefficients of Transitory Values of the Crampton Index with Wet Bulb Temperature and with Corrected Effective Temperature

Subject and (Sex)	Resting		Step-climbing		Crampton Index correlated with:
	R.H. 60 per cent	R.H. 80 per cent	R.H. 60 per cent	R.H. 80 per cent	
A (F)	−0·619	−0·917	−0·914	−0·945	Wet bulb temperature
	−0·790	−0·930	−0·860	−0·880	Corrected Effective Temperature
B (M)	−0·815	−0·901	−0·912	−0·966	W.B.T.
	−0·856	−0·896	−0·947	−0·980	C.E.T.
C (M)	−0·897	−0·936	−0·965	−0·968	W.B.T.
	−0·919	−0·937	−0·965	−0·990	C.E.T.
D (M)	−0·969	−0·933	−0·919	−0·933	W.B.T.
	−0·987	−0·941	−0·971	−0·898	C.E.T.
E (M)	−0·825	−0·930	−0·941	−0·933	W.B.T.
	−0·834	−0·918	−0·955	−0·854	C.E.T.
F (F)	−0·870	−0·813	−0·945	−0·945	W.B.T.
	−0·912	−0·954	−0·737	−0·947	C.E.T.

A numerical value of the correlation coefficient greater than 0·455 is significant at the 5 per cent level. There was no evidence to suggest that the Crampton Index was more highly correlated with the Corrected Effective Temperature (C.E.T.) than with the wet bulb temperature. Table 3 gives the values of the rate of decrease in the Crampton Index per 1·0°F rise in wet bulb temperature for all the subjects, and Table 4 gives the values of the rate of decrease in the Crampton Index per 1·0°F rise in the C.E.T.

Table 3. Rate of Decrease of the Crampton Index per 1·0°F Increase in the Wet Bulb Temperature

Subject	Resting		Step-climbing	
	R.H. 60 per cent	R.H. 80 per cent	R.H. 60 per cent	R.H. 80 per cent
A	1·51	1·66	2·56	2·63
B	1·99	2·14	3·37	2·66
C	1·60	2·55	2·06	2·48
D	2·59	1·79	3·24	3·16
E	1·33	2·40	1·98	2·76
F	2·79	2·46	2·39	2·69

An analysis of variance was carried out to assess the significance of the effect of the two humidity levels and of step-climbing on the rate of fall in the Crampton Index. From the results summarized in Tables 3 and 4 it was found that there is no significant difference between subjects or between the two levels

of humidity used. With both levels of humidity, however, the differences in the rates of fall in the Crampton Index between exercise and rest are significant at the 1 per cent level. The analysis indicated also that, generally speaking, the rate of decrease in the Crampton Index was the same for all the subjects.

Table 4. Rate of Decrease of the Crampton Index per 1·0°F Increase in the Corrected Effective Temperature

Subject	Resting		Step-climbing	
	R.H. 60 per cent	R.H. 80 per cent	R.H. 60 per cent	R.H. 80 per cent
A	1·78	1·71	2·72	2·29
B	1·63	1·69	2·71	2·71
C	1·42	2·32	1·85	2·71
D	2·09	1·53	2·70	2·45
E	1·16	1·98	1·76	2·09
F	2·19	1·91	1·91	2·23

3.2. *Thermal Sensations in Relation to Corrected Effective Temperature*

The algebraic sum of the numerical equivalents of the thermal sensations of heat and moisture was considered by Crowden and Lee (1940) to be a good, although empirical, index of overall warmth-comfort. These sums were computed for each subject, correlations calculated and an analysis of variance then carried out as for the data on thermal environment. The correlation coefficients between thermal sensation index and C.E.T. were significant for each subject, the lowest and the highest values being +0·597 and +0·987 with a mean value for all experiments of +0·913.

Muscular work (step-climbing) did not produce a statistically significant effect upon the rate of change of the values of the summated sensations of heat and moisture per 1·0°F increase in C.E.T. The mean values for all subjects, resting and step-climbing, of the regression coefficients of summated sensations upon C.E.T. were +0·43 and +0·47 respectively, with a standard deviation of 0·037.

3.3. *Changes in the Crampton Index in Relation to Thermal Sensations*

From the data summarized in Table 4 it can be calculated that, for all subjects, the mean fall in Crampton Index was 17·8 points per 10°F increase in C.E.T. in the resting state, and 23·4 points per 10°F increase for step-climbing at regular time intervals.

The observations of the thermal sensations were used as a basis for the assessment of the comfort zone, according to the following scale: the range within which the subjects felt comfortable (=0), comfortably warm (=+1) or comfortably cool (=−1). In this way the corresponding ranges of Crampton Index and C.E.T. were computed, and the results are given in Table 5. From this table it is seen that the greater the range of temperature within which the subject felt comfortable, according to thermal sensations, the wider becomes the corresponding Crampton Index Range. Thus for subject F a

C.E.T. range of 20·8°F was associated with a Crampton Index range of 30, whilst a C.E.T. range of 7·4°F was found when the C.I. range was only 15.

Table 5. Comfort Zone Ranges for Crampton Index (C.I.) and Corrected Effective Temperatures (C.E.T.) for All Subjects. Within the ranges shown all subjects felt comfortable (0), comfortably warm (+1) or comfortably cool (−1)

Subject	Resting		Step-climbing		Range of:
	R.H. 60 per cent	R.H. 80 per cent	R.H. 60 per cent	R.H. 80 per cent	
A	80–65 69·0–76·9	70–75 77·0–81·2	75–65 72·0–74·0	75–65 71·2–73·8	C.I. C.E.T.
B	50–45 68·2–71·0	50–20 71·2–83·2	60–15 56·5–74·2	75–40 65·0–76·1	C.I. C.E.T.
C	70–60 56·5–69·8	55 68·2–73·0	55–20 56·4–75·3	55–25 59·5–79·0	C.I. C.E.T.
D	65–35 64·3–75·0	60–55 57·9–69·0	65–60 64·4–69·8	60–55 63·6–70·2	C.I. C.E.T.
E	90–85 64·8–75·7	85–60 67·8–77·5	65 66·0–69·0	70–60 57·5–69·5	C.I. C.E.T.
F	75–60 55·7–73·8	80–65 66·2–80·2	85–55 55·4–76·2	65–50 60·1–67·5	C.I. C.E.T.

The changes in the Crampton Index from the comfort zone values to those recorded with the onset of thermal discomfort have been investigated. This was done by taking the decrease from the mean comfort zone value of the Crampton Index to the first value of the Crampton Index recorded when the subject experienced a thermal sensation of +3 (too warm) on the heat scale or +3 (too humid) on the moisture scale. The results are shown in Table 6. Expressed in terms of the Crampton Index the decreases give a mean value of about 25 with a standard deviation of about 10.

Table 7 gives the values for the rate of decrease in the Crampton Index per unit increase in the sensation of warmth. The results indicate that there is a

Table 6. Crampton Index Decreases from Mean Comfort Zone Value Associated with the Onset of Thermal Discomfort

Subject	Mean Crampton Index within the comfort zone	Mean decrease of Crampton Index associated with 3 on the heat or moisture sensation scales
A	65	16·7
B	37·5	20·0
C	47·5	23·75
D	50·0	10·0
E	75·0	31·25
F	67·5	40·0

significant correlation between changes in Crampton Index and changes in the algebraic sum of the numerical values assigned to the sensations of heat and moisture. The mean value for the correlation coefficient was −0·820 for a total of 391 observations, with a range of −0·746 to −0·923 for the 6 subjects.

Table 7. Rate of Decrease of the Crampton Index Per Unit Increase in the Sensation of Warmth

Subject	Rate of decrease of Crampton Index per unit increase in sensation of warmth
A	2·75
B	4·55
C	4·06
D	4·02
E	4·39
F	4·35

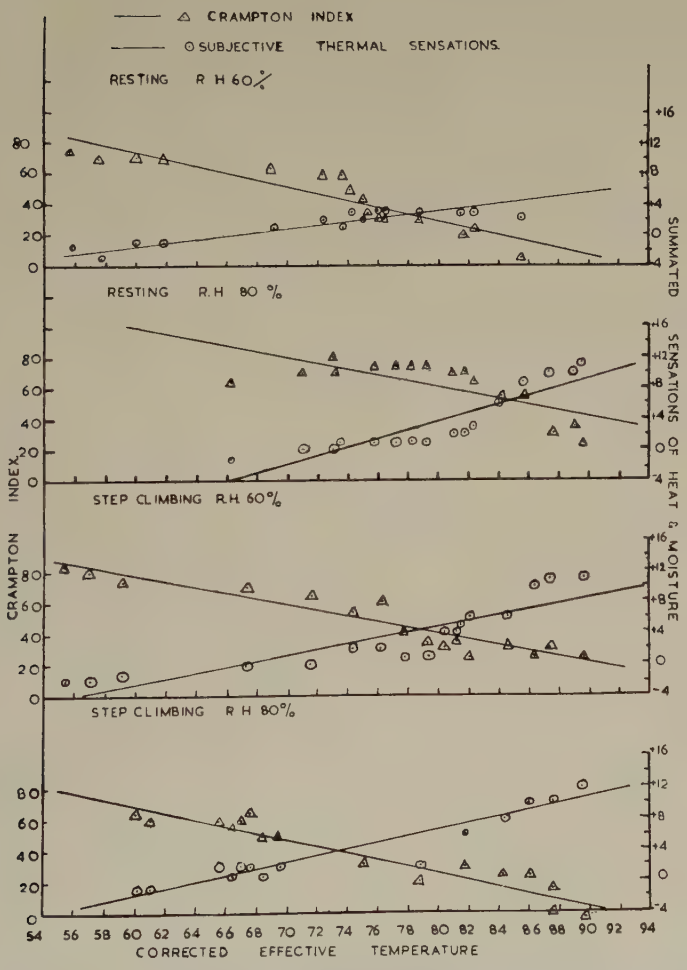
CRAMPTON INDEX
SUBJECTIVE THERMAL SENSATIONS
RESTING R H 60%
RESTING R.H 80 %
STEP CLIMBING R.H 60%
STEP CLIMBING R H 80%
CRAMPTON INDEX
SENSATIONS OF HEAT & MOISTURE
SUMMATED
54 56 58 60 62 64 66 68 70 72 74 76 78 80 82 84 86 88 90 92 94
CORRECTED EFFECTIVE TEMPERATURE

Figure 2. Changes in the Crampton Index and in the summated sensations of heat and moisture during rest and activity in relation to Corrected Effective Temperature at 60 per cent and 80 per cent relative humidity. (Subject F.)

The changes of Crampton Index and of summated sensations of heat and moisture for the four experimental conditions for subject F are given in Fig. 2. The results for the other subjects are similar. The relation between the Crampton Index and subjective sensations of heat and moisture (subject D) are given in Fig. 3.

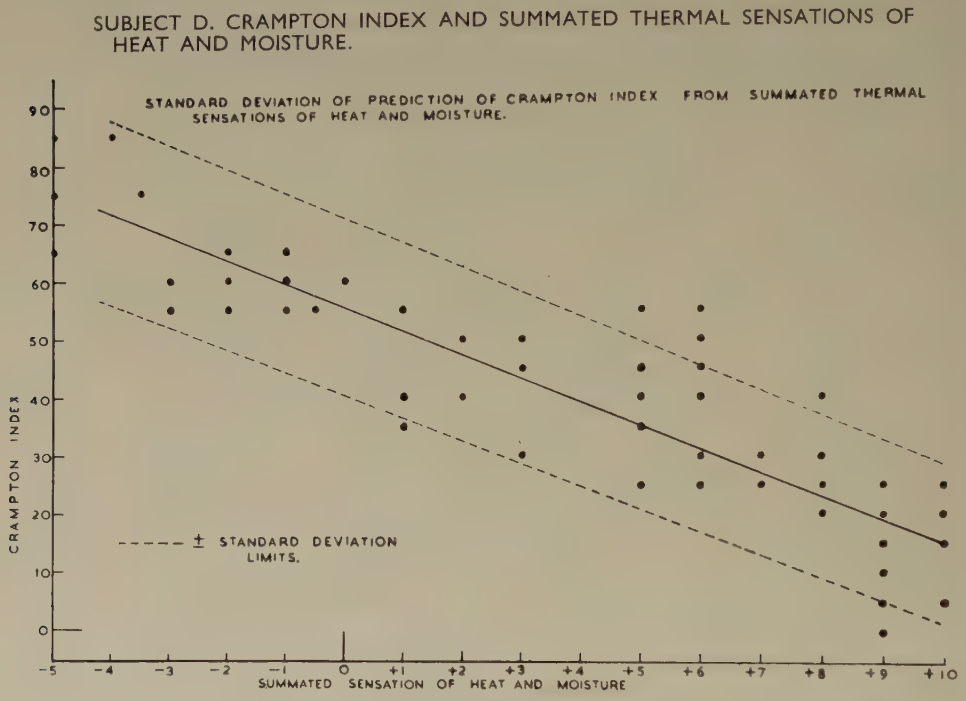
SUBJECT D. CRAMPTON INDEX AND SUMMATED THERMAL SENSATIONS OF HEAT AND MOISTURE.
STANDARD DEVIATION OF PREDICTION OF CRAMPTON INDEX FROM SUMMATED THERMAL SENSATIONS OF HEAT AND MOISTURE.
CRAMPTON INDEX
90
80
70
60
50
40
30
20
10
0
± STANDARD DEVIATION LIMITS.
-5 -4 -3 -2 -1 0 +1 +2 +3 +4 +5 +6 +7 +8 +9 +10
SUMMATED SENSATION OF HEAT AND MOISTURE

Figure 3. The Crampton Index changes in relation to sensations of heat and moisture. (Subject D.)

§ 4. Discussion

Crampton (1920) stated that most adults in normal health have a Crampton Index between 50 and 100. He considered that values of his index below zero were conclusive of 'impaired circulation, toxaemia or severe physical disturbance'. Trends of change in the index are of greater physiological significance than single determinations since healthy people have widely divergent Crampton Index values. While daily variations were observed in individual subjects used in the present study, these were not greater than 5 to 10 points under ordinary conditions, i.e. before exposure to environmental heat.

Crampton (1920) considered his index to be an 'estimate of the influence which returns blood to the heart when an upright position is actively assumed'. Winslow and Herrington (1949) consider the Crampton Index to be useful as a measure of the efficiency of the cardiac and circulatory adjustments to meet various stresses. A low value of the Index is taken to be indicative of decreased adaptation of the circulation to the increased load placed upon it by physical stress. The efficiency of these adjustments is measured in part by the rise in systolic blood pressure on standing. However, it is known that blood pressure changes are related to variations in cardiac output and peripheral

vascular resistance. The peripheral vascular resistance is predominantly determined by the tone of blood vessels in the skin and splanchnic areas. The vasomotor tone of the splanchnic vessels is very variable. It can be altered by such conditions as external heat and cold, smoking and psychological factors.

Crampton's original concept of a low value indicating decreased vasomotor tone did not take into account the effect of possible increased capacity of the vascular bed due to dilatation of the skin vessels in a warm environment. The greater the dilatation of these vessels, the less will be the effect of the compensation afforded by the vaso-constrictor activity of the splanchnic vessels. Thus it follows that a low Crampton Index does not necessarily indicate decreased vasomotor tone within the splanchnic vascular system.

The initial resting values of the Crampton Index for the different subjects varied between 50 and 90; such values are within the normal limits given by Crampton. The rate of fall of the Crampton Index due to increased environmental heat was not dependent upon the initial resting value for the six subjects. The lowest values of the Crampton Index were recorded at the end of each experiment when the environmental heat was at a maximum. At such times the subjects showed considerable respiratory embarassment and symptoms of heat strain consistent with their thermal sensations. The onset of symptoms of heat strain was considered to occur when the subjects expressed thermal sensations of +3 on the heat scale and +3 on the moisture scale, and was apparent at a lower C.E.T. when the subjects were step-climbing (78·5°F) than when they were at rest continuously throughout the experiment (82·0°F). These temperatures are the mean values of 12 observations and the difference (3·5°F) was found to be statistically significant.

Crampton considered that low values of his index meant diminished physical efficiency. The results show that there was a gradual decrease in the Crampton Index as the environmental warmth increased. This gradual decrease in the Crampton Index indicates that the onset of heat strain and impairment of physical efficiency took place gradually under the present experimental conditions.

Houghten and Yagloglou (1923 a) demonstrated that certain physiological reactions such as pulse rate, rectal temperature changes and loss of body weight, were more closely correlated with Effective Temperature than with the wet bulb or dry bulb temperatures taken separately. The Effective Temperature (Houghten and Yagloglou 1923 b, Yagloglou and Miller 1925) is a single index evaluated from combinations of temperature, humidity and air movement, and the scale was built up on the basis of the subjective thermal impressions of a large number of subjects. Bedford (1946) considered that sources of radiation should be taken into account in the evaluation of this temperature index, and suggested that the dry bulb temperature readings should be replaced by globe thermometer readings, the scale then being called the 'Corrected Effective Temperature Scale'. Changes in the Crampton Index were considered therefore with respect to changes in C.E.T.

Crowden and Lee (1940) showed that there was a high degree of correlation between the summated sensations of heat and moisture and the total heat of the air. This was for resting subjects. The present experiments show that the degree of correlation is not affected by exercise.

Subject A showed a slower rate of decrease of Crampton Index with increase in sensations of warmth than the other five subjects (see Table 7). Her

reactions with respect to changes in the Crampton Index with C.E.T. were comparable to those of the others but changes in summated sensations with respect to C.E.T. were significantly higher. This is considered to be due to the heavier clothing worn by this subject.

§ 5. Conclusions

The experiments have demonstrated that changes in the Crampton Index can be used as a measure of the impaired adaptation of the circulation in individuals exposed to conditions of increasing environmental temperature and humidity over a range of 55 to 100°F dry bulb and 48 to 94°F wet bulb. The absolute values of the index are not considered to be of use in the assessment of physiological stress since a wide range of values was found for individual subjects at rest. Changes in the Crampton Index values, however, were found to be closely associated with changes in sensations of heat and moisture, and marked decreases, e.g. 40 to 50 in the index, were associated with symptoms of thermal stress, such decreases being aggravated by muscular work.

This investigation formed a part of a programme of research on requirements for comfort and health in housing carried out by the Department of Applied Physiology, London School of Hygiene and Tropical Medicine aided by a grant from the Department of Scientific and Industrial Research.

L'article décrit des expériences dans lesquelles les valeurs de l'indice de Crampton, obtenu à partir de la pression sanguine et de la fréquence cardiaque observées lors du passage de la position couchée à la station debout, ont été étudiées en fonction de conditions thermiques ambiantes et en fonction des sensations thermiques de sujets au repos et au travail. Il ressort de ces expériences que l'indice de Crampton peut utilement servir à apprécier les réactions individuelles à la charge thermique ambiante.

Es werden Experimente beschrieben, in welchem Crampton-Index-Werte (abgeleitet aus Messungen der Aenderung des Blutdruckes und der Pulsfrequenz nach Aenderung der Körperstellung) zu heissen klimatischen Bedingungen und zu den Wärme-Empfindungen ruhender und arbeitender Personen in Beziehung gesetzt wurden. Die Experimente beweisen, dass Aenderungen des Crampton-Index für die Beurteilung individueller Reaktionen auf zunehmende Hitze verwertet werden können.

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THE ESTIMATION OF THE TRANSFER FUNCTION OF A HUMAN OPERATOR BY A CORRELATION METHOD OF ANALYSIS

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This paper records an experiment to determine the transfer-function of a human operator, when he is acting as an element in a closed-loop control-system, by a method of analysis which is based on correlation functions.

The theoretical analysis shows that under certain conditions the transfer-function of the operator can be defined by an integral equation that relates a cross-correlation function of his response to an observed ' error ', to the auto-correlation function of that ' error '.

It is shown how this integral equation can be solved approximately by a technique which uses an electronic analogue-computer. In this instance such a computer has been used to determine the parameters that will ' best fit ' an assumed form of transfer-function to the recorded data.

§ 1. Introduction

The development of manually-operated, closed-loop, control-systems, such as aircraft flight-controls and gun-laying systems, has led engineers to enquire whether the human-operator who is included in the closed-loop and therefore is an essential element in determining the stability of the system, can be represented by a 'black box' with certain dynamic transfer-characteristics. That is to say that the operator has adopted a consistent type of manual response to an observed signal in a particular control situation. If this were so it would be possible for an engineer to use this information for the design of a control-system for a particular task, and it would give him specific information about the sensitivity and degree of stabilization that he would have to provide in his control equipment.

List of Principal Symbols

$\theta_1(t)$ = Input signal representing the motion of a target.

$\theta_0(t)$ = Output signal from a manually operated controller.

$\epsilon(t) = \theta_1(t) - \theta_0(t)$ = error seen by the operator.

$H(t)$ = Handle movement.

$N(t)$ = Random disturbance.

The Laplace transforms of the above time-functions are written $\theta_1(p)$, $\theta_0(p)$ . . . .

$$W_1(p) = \frac{\dot{H}}{\epsilon}(p) = \frac{K(1+pT_1)\exp(-pT)}{p^2+2\alpha p+\omega_0{}^2}$$

= An assumed form of linear transfer-function relating handle speed to observed error.

K = Scalar constant

T_1 = Coefficient of handle speed to rate of change of error.

T = Time delay.

$\frac{1}{p^2+2\alpha p+\omega_0{}^2}$ = Transfer-function analogous to that of a mechanical system comprising a mass, spring and dashpot.

ω_0 = Undamped, natural angular frequency.

α = Damping coefficient; $\alpha/\omega_0 = 1$, corresponds to critical damping.

$w_1(t)$ = Time function response of the above system to a unit impulse.

$W_2(p)$ = Transfer-function of the controller.

$w_2(t)$ = Impulse response of the controller.

$A(\tau)$ = Auto-correlation function.

$C(\tau)$ = Cross-correlation function.

Some experimental work to determine the dynamic characteristics of a human operator was done in connection with manually controlled, electrically operated gun turrets on tanks by Professor A. Tustin, and was reported in 1947. Briefly these experiments consisted of moving a target in a complex periodic manner, and as an operator attempted to track this target, by following it with a sight, records were taken of the 'error' between the target and the line of the sight, and also of the corresponding displacement of the handle which actuated the control system. The records of error and handle position were subjected to Fourier analysis for those frequencies that were present in the target signal and from these results vector relationships between handle position and error were obtained for a few frequencies; that is, a few points on the frequency-response characteristic of the operator were determined. From these it was deduced that the operator's response could be represented by an equation of the form

$$\dot{H}(t+T)=K_1\epsilon(t)+K_2\frac{d\epsilon(t)}{dt};$$

that is, the speed of handle movement delayed by T seconds was related to the magnitude of the error and to the derivative of the error.

The test situation for these experiments constituted a pursuit task; that is, one in which the operator could see both the motion of the target and the magnitude of the error; hence it was not strictly correct to presume that the operator derived information from the error signal alone. He can, in fact, derive some information from the target motion, but it is impossible to determine how much he derives from each.

To overcome this difficulty a further series of experiments was begun under Professor Tustin at Birmingham University, in which only the 'error' signal was displayed to the operator and he was told to keep the error as small as possible whilst the target was moved. This is a 'compensatory' task, and the only information the operator can receive is from the error signal.

Records of error and handle position for complex periodic target-signals were again subjected to Fourier analysis with the object of determining a frequency-response characteristic for the operator, but inconsistent results were obtained at higher target-frequencies of the order of 0·3 c/s.

An alternative method of analysis was sought, which is that now reported, and this is based on the process of the correlation of the records of error and handle position. It is shown that in a linear-system the auto-correlation

function of the error would be related to the cross-correlation function of the error and handle position, by the transfer-function of the operator. The technique which has been adopted has been to calculate these auto- and cross-correlation functions for the experimental data, and then find, with the aid of an analogue-computer, a transfer-function that will best relate one to the other. This transfer-function is the best linear approximation to the characteristics of the operator in this particular test situation.

§ 2. The Experimental Situation

In these experiments a human operator was included in the closed-loop control scheme represented diagrammatically by Fig. 1, to which the input signal $\theta_1(t)$ was a random time function. The 'error' signal $\epsilon_1(t)$ was displayed to the operator as the deflection of a pointer. The operator was given instructions to keep this pointer on zero by appropriate movements of a control lever, which in turn fed a proportional voltage-signal to an integrator, the output $\theta_0(t)$ of which was fed back in opposition to $\theta_1(t)$. In this way the operator became an element in an error-actuated closed-loop control-system. This system can be regarded as being equivalent to a practical tracking situation in which, say, a motor-operated sight is controlled by a human operator through the agency of a displacement/speed type of controller; that is, one for which $\dot{\theta}_0$ is proportional to handle displacement H.

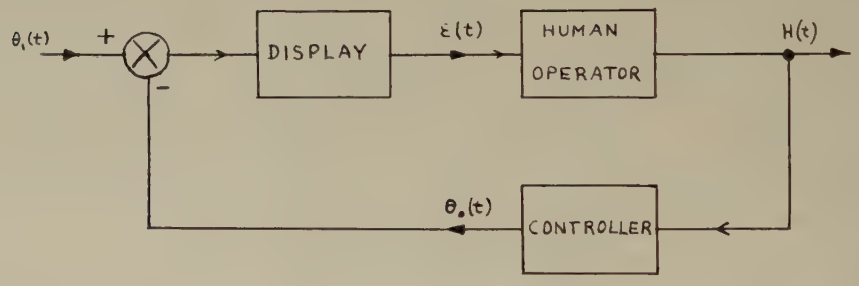


Figure 1. Closed-loop control-scheme including a human operator.

In these particular tests the operator sat facing the display, with his elbow down by his side and resting on a pad immediately above the pivot of the lever for operating the controller. His fore-arm was extended with his hand gripping the controller handle and corrective movements were made by lateral movements of the fore-arm. Movements of the error pointer subtended a maximum angle of approximately 1/20 radian at the operator's eye.

In a situation such as this it takes about six to eight hours of training before the operator reduces his error of following to a consistent minimum level, and it is for conditions such as these that it is of interest to discover whether his behaviour can be represented by a linear transfer-function between the observed error $\epsilon(t)$ and the handle movement $H(t)$.

The method which is about to be described can be used to determine the most general form of transfer-function for a human operator, but it has been used

in this case to determine the best parameters for a transfer-function of prescribed form, namely

$$\frac{\dot{H}}{\epsilon}(p) = \frac{K(1+pT_1)\exp(-pT)}{p^2+2\alpha p+\omega_0{}^2}$$

which is thought to contain sufficient terms to represent the most probable characteristics of a human operator.

§ 3. Theoretical Analysis

A feature of records of $\epsilon(t)$ and $H(t)$, such as those shown in Fig. 2, is that there are components present that appear to be unrelated to the stimulus $\theta_1(t)$; this is most apparent when the stimulus is a simple periodic signal. It is presumed that these disturbances are due to the failure of the operator to respond linearly to the observed error $\epsilon(t)$.

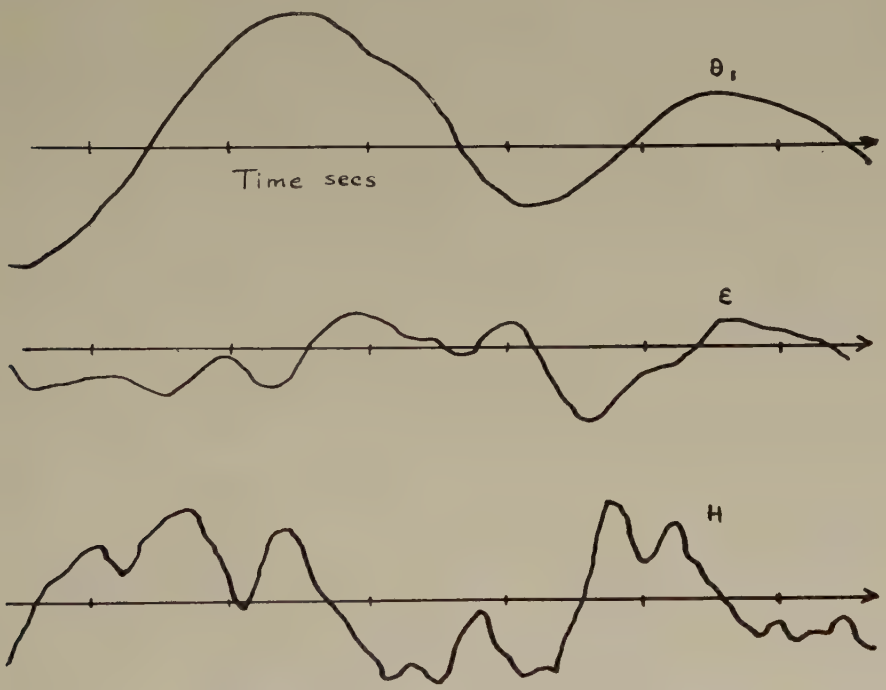


Figure 2. Typical records of θ_1, ϵ, and H.

To formulate this problem analytically it has been assumed that the operator's response may be represented by

(*a*) a linear transfer-function $W_1(p)$ operating upon the error, and

(*b*) a source of random disturbances $N(t)$ by which we attempt to represent his errors of judgement and his departures from linear behaviour.

Thus the displacement of the controller handle may be defined in operational form as

$$H(p) = W_1(p) \,.\, \epsilon(p) + N(p) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

and the closed-loop control system may be represented by the dependence diagram shown in Fig. 3.

The object of this investigation is to determine the operator's transfer-function $W_1(p)$ from records of $\epsilon(t)$ and $H(t)$. However, in eqn. (1) we have one equation with two unknowns, $W_1(p)$ and $N(p)$, and it is apparently impossible to determine $W_1(p)$. If we resort to correlation analysis, and cross-correlate $\epsilon(t)$ with $H(t)$ and presume that $N(t)$ is a random function, then subject to certain restrictions, it is possible to estimate $W_1(p)$.

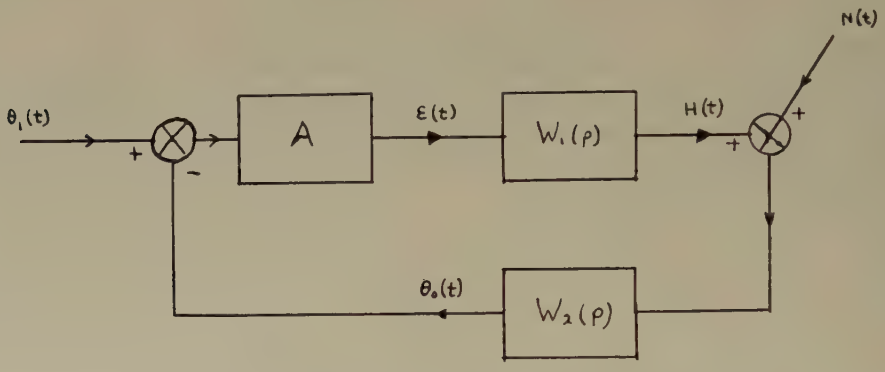


Figure 3. Dependence diagram for the control-system.

3.1. *The Analysis of a System with Two Inputs:* $\theta_1(t)$ *and* $N(t)$

Using time-function notation, eqn. (1) may be written as

$$H(t)=\int_0^\infty w_1(x)\,.\,\epsilon(t-x)\,dx+N(t).$$

If we now cross-correlate $\epsilon(t)$ with $H(t)$, that is evaluate the function

$$C(\tau)=\lim_{T\to\infty}\frac{1}{2T}\int_{-T}^{+T}\epsilon(t-\tau)\,.\,H(t)\,.\,dt$$

we obtain

$$C(\tau)=\lim_{T\to\infty}\frac{1}{2T}\int_{-T}^{+T}\epsilon(t-\tau)\left\{\int_0^\infty w_1(x)\,.\,\epsilon(t-x)\,dx+N(t)\right\}dt$$

and after interchanging the order of integration and allowing $T\to\infty$ we obtain

$$C(\tau)=\int_0^\infty w_1(x)\,.\,A(\tau-x)\,dx+C_{\epsilon N}(\tau) \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where

$$A(\tau)=\lim_{T\to\infty}\frac{1}{2T}\int_{-T}^{+T}\epsilon(t-\tau)\,.\,\epsilon(t)\,.\,dt$$

which is defined as the auto-correlation function of the error, and

$$C_{\epsilon N}(\tau)=\lim_{T\to\infty}\frac{1}{2T}\int_{-T}^{+T}\epsilon(t-\tau)\,.\,N(t)\,dt$$

which is defined as the cross-correlation function between the error $\epsilon(t)$ and the disturbance $N(t)$.

In eqn. (2) we still have an equation with two unknowns $w_1(x)$ and $C_{\epsilon N}(\tau)$, but it can be simplified on further study of the function $C_{\epsilon N}(\tau)$.

The error signal $\epsilon(t)$ may be considered to be composed of two components: $\epsilon_1(t)$ due to the stimulus $\theta_1(t)$, and $\epsilon_N(t)$ due to the disturbance $N(t)$, and as we have assumed $\theta_1(t)$ and $N(t)$ are uncorrelated, then

$$C_{\epsilon N}(\tau) = \lim_{T\to\infty} \frac{1}{2T}\int_{-T}^{+T} \epsilon_N(t-\tau)\,.\,N(t)\,.\,dt.$$

An important feature of this latter cross-correlation function is that $N(t)$ is the cause of $\epsilon_N(t)$, and cross-correlation for positive values of τ represents a measure of the average linear dependence of values of $\epsilon_N(t)$ that *precede* the cause $N(t)$. Thus $C_{\epsilon N}(\tau)$ for $\tau > 0$ can be expected to be small. Conversely, values of $C_{\epsilon N}(-\tau)$, which relate to error signals that occur later than the cause $N(t)$, will be relatively large. Hence the contribution of $C_{\epsilon N}(\tau)$ to $C(\tau)$ will be largely to the left of the origin, as shown in Fig. 4, and it will be permissible to assume that

$$C(\tau) = \int_0^\infty w_1(x)\,.\,A(t-x)\,dx \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

for values of $\tau > \tau_1$ where τ_1 is a value sufficiently large for $C_{\epsilon N}(\tau_1)$ to be negligibly small in comparison with the value of $C(\tau_1)$ given by eqn. (3).

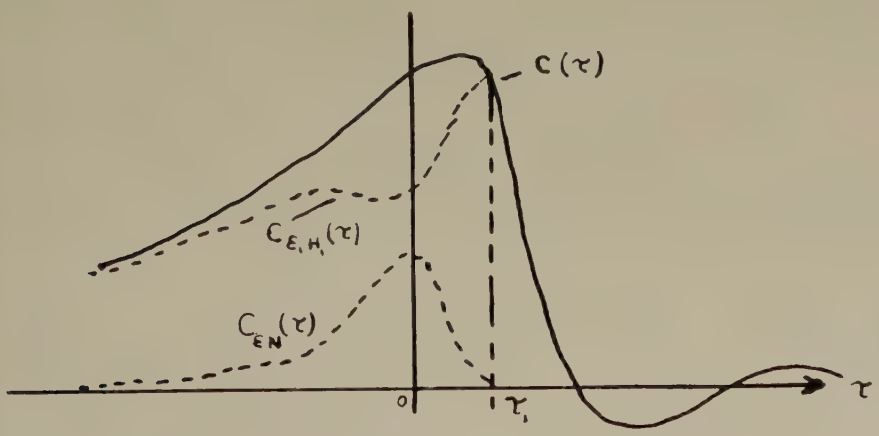


Figure 4. Composition of the cross-correlation function $C(\tau)$.

A useful way of regarding the function $C_{\epsilon N}(\tau)$, which gives an indication of the order of τ_1, may be obtained by considering the response of the theoretical system shown in Fig. 3 to $N(t)$ alone.

Assume that the transfer-function between $N(t)$ and the error $\epsilon(t)$ is

$$W_3(p) = \frac{A\,.\,W_2(p)}{1 + A\,.\,W_1(p)\,.\,W_2(p)};$$

then the error signal may be defined by

$$\epsilon_N(t) = \int_0^\infty w_3(x)\,.\,N(t-x)\,dx.$$

The cross-correlation function between $N(t)$ and the error signal may now be defined as

$$C_{N\epsilon}(\tau) = \lim_{T\to\infty} \frac{1}{2T}\int_{-T}^{+T} N(t-\tau)\left\{\int_0^\infty w_3(x)\,.\,N(t-x)\,dx\right\} dt$$

and after interchanging the order of the integration and allowing $T \to \infty$, we obtain

$$C_{N\epsilon}(\tau) = \int_0^\infty w_3(x) \, . \, A_N(\tau - x) \, dx.$$

If the disturbance $N(t)$ constituted 'white noise', its auto-correlation function $A_N(\tau)$ would be a delta function at $\tau = 0$ and $C_{N\epsilon}(\tau)$ would then equal $w_3(\tau)$, which is zero for $\tau < 0$. Hence $C_{N\epsilon}(\tau) = 0$ for $\tau < 0$, and $C_{\epsilon N}(\tau) = C_{N\epsilon}(-\tau)$, with which we are concerned, will be zero for $\tau > 0$. If the bandwidth of the disturbance is restricted, and its amplitude/frequency spectrum departs from the constant amplitude/frequency spectrum of 'white noise', so the auto-correlation function $A_N(\tau)$ will broaden and $C_{\epsilon N}(\tau)$ will no longer be zero for small positive values of τ.

If $A_N(\tau)$ could be determined separately it would be possible to make a judicious estimate of the value of τ_1 for which $C_{\epsilon N}(\tau_1)$ would be negligibly small in comparison with $C(\tau_1)$; in this situation, however, this is not possible.

Thus the analytical problem that remains is how to find the function $w_1(x)$ that will satisfy the equation

$$C(\tau) = \int_0^\infty w_1(x) \, . \, A(\tau - x) \, dx$$

for values $\tau > \tau_1$, when some uncertainty exists about the value of τ_1.

§ 4. The Approximate Solution of the Integral Equation

The approximate solution of the integral equation is based on the similarity of its form with that of the integral equation relating the output voltage $V_0(t)$ of a network in response to an input voltage $V_i(t)$ namely

$$V_0(t) = \int_0^\infty Y(x) \, . \, V_i(t - x) \, dx$$

where $Y(x)$ is the weighting-function or indicial-response of the network.

Thus if the auto- and cross-correlation functions $A(\tau)$ and $C(\tau)$ are reproduced as equivalent voltages $V_A(\tau)$ and $V_C(\tau)$, it is possible to apply $V_A(\tau)$ to an electronic analogue-computer with adjustable parameters, which may then be adjusted until the output voltage $V_a(\tau)$ of the analogue most closely matches the voltage $V_C(\tau)$ corresponding to the cross-correlation function. The transfer-function of the analogue, when this 'best fit' has been achieved, is then an approximation to the required function $W_1(p)$.

The appropriate weighting-function, $w_1(x)$, can be derived directly from the analogue since it is simply the response of the analogue to a unit impulse of input voltage. The frequency-response characteristics of the function, that is $W_1(j\omega)$, are obtained just as easily by direct measurements of the output-voltage of the analogue in response to a sinusoidal input-voltage.

The general computational scheme is shown in Fig. 5. Further details of this analytical technique are given by Henderson and Pengilley (1958).

In this particular problem, the technique adopted was to fit the cross-correlation function $V_C(\tau)$ first of all for large values of τ only, and then over a progressively extending region with τ approaching zero from the positive side, until such time as the fit began to degenerate. This was presumed to occur in the region of $\tau < \tau_1$.

The auto- and cross-correlation functions $A(\tau)$ and $C(\tau)$ are determined in practice from the time-series equations

$$A(m) = \frac{1}{N} \sum_{n=1}^{n=N} \epsilon(n-m) \cdot \epsilon(n)$$

and

$$C(m) = \frac{1}{N} \sum_{n=1}^{n=N} \epsilon(n-m) \cdot H(m)$$

where $\epsilon(n)$ and $H(n)$ are the ordinates of time series representing the time functions $\epsilon(t)$ and $H(t)$; $n = t/\Delta$, where Δ is the time interval separating successive ordinates. Hence the value of the correlation lag, τ, corresponding to m, is $\tau = m\Delta$.

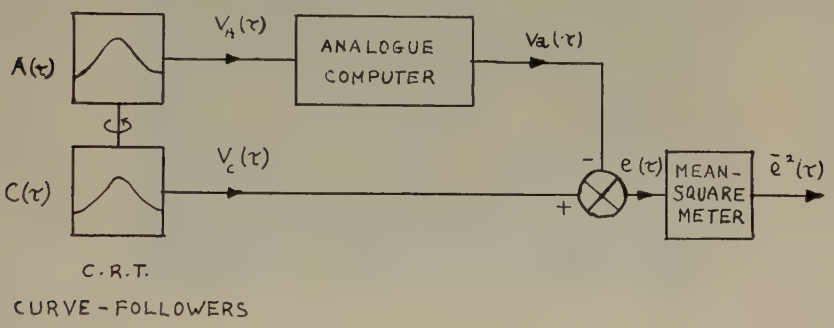


Figure 5. Scheme for the experimental determination of $W_1(p)$.

Unless special analogue computing equipment is available for determining these correlograms, the most expedient course is to have them calculated on an electronic digital-computer, and computing programmes for the process of correlation are available for most of the commercial computers.

There are three general restrictions on the data to be used for correlation analysis:

(*a*) The bandwidth of the frequency-spectrum of the input should ideally be wider than the bandwidth of the system under examination, otherwise there is insufficient high frequency information present in the output records from which to determine the system response at high-frequencies with any exactitude. In this particular case, however, the 'self-generated' disturbances $N(t)$ make a useful contribution to the bandwidth of the observed error signal $\epsilon(t)$.

(*b*) The records should be long enough to constitute a stationary time series, and a rough guide is that the record should be about ten times as long as the lowest periodicity evident in the input signal.

(*c*) The sampling interval Δ must be so chosen that the cut-off frequency $fc = 1/2\Delta$, which it imposes on the data, is greater than the significant bandwidth of the system under examination.

§ 5. Experimental Results

Typical normalized auto- and cross-correlograms, calculated by a digital computer from records of $\epsilon(t)$ and $H(t)$ of 45 sec duration, are shown in Fig. 6.

The stimulus $\theta_1(t)$ in these experiments was derived from a set of random numbers (Wold 1948) which were subsequently smoothed and reproduced as a voltage by a cathode-ray tube curve-follower.

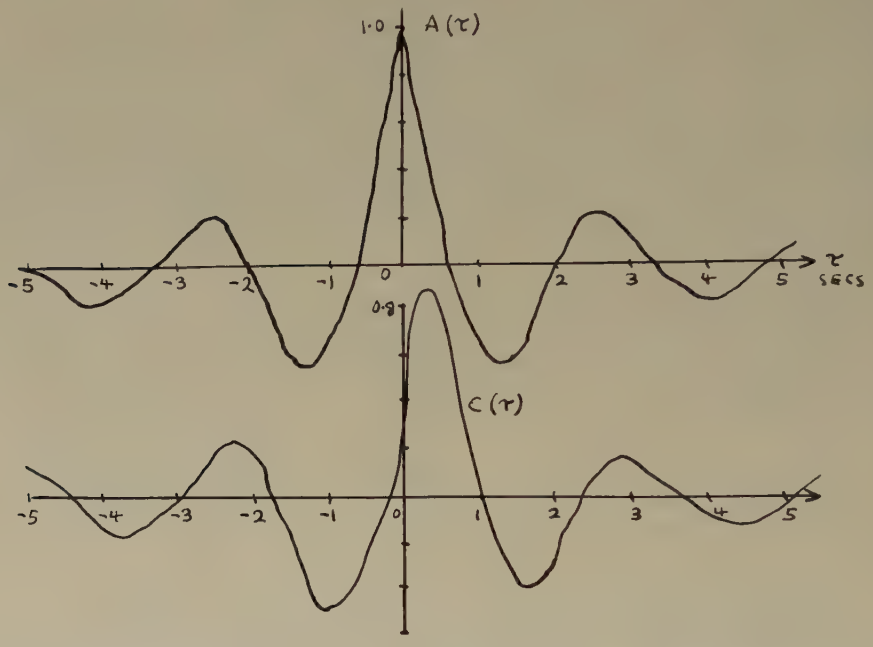


Figure 6. Typical auto- and cross-correlograms for manual tracking situation (case 1).

The correlograms were also reproduced as voltages $V_A(\tau)$ and $V_C(\tau)$ by cutting cams with profiles to correspond to $A(\tau)$ and $C(\tau)$ and rotating them in front of cathode-ray tube curve-followers. Time delays were produced simply by shifting the origin of the auto-correlation function cam with respect to that of the cross-correlation function cam. The voltage $V_A(\tau)$ was applied repetitively to an analogue computer with variable parameters, which were adjusted until the output voltage $V_a(\tau)$ of the analogue matched most closely the cross-correlogram signal $V_C(\tau)$. Provision was actually made to measure the mean-square-error of fit $\overline{e^2(\tau)} = \overline{(V_C(\tau) - V_a(\tau))^2}$, but this is in practice a relatively insensitive criterion of fit and it proved to be more expedient to fit the curves visually. The quality of fit that can be obtained is shown in Fig. 7.

Transfer-functions were determined experimentally for two sets of correlograms, hereafter referred to as cases (1) and (2), for which the display gain-ratio $A_2/A_1 = 2$. That is, in case (2) the display gain is twice as large as in case (1) and assuming that the operator adopts as high a gain as is possible, consistent with a subjectively satisfactory closed-loop characteristic, then one would expect that in case (2) the operator would have to halve his d.c. gain.

In each case the correlograms were fitted experimentally to determine the best parameters for a transfer-function of the form

$$\frac{\dot{H}}{\epsilon}(p) = \frac{K(1+pT_1)\exp(-pT)}{p^2 + 2\alpha p + \omega_0{}^2}. \qquad (4)$$

This form was chosen because it appears to be capable of representing the probable dynamical behaviour of an operator engaged in the continuous process of manual tracking. The factor T represents the operator's time delay to a stimulus; the factor T_1 represents his ability to respond to both the error and the rate of change of the error, and the quadratic factor $1/p^2+2\alpha p+\omega_0{}^2$ is an attempt to represent the 'motor' system comprising the muscles and the inertia of the arm.

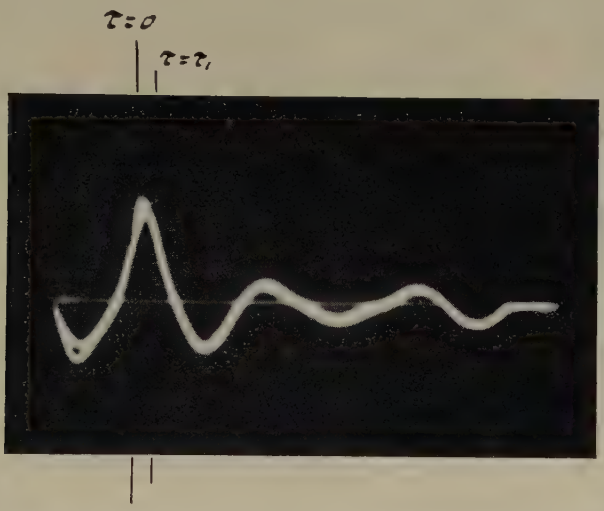


Figure 7. Oscillogram showing a fit of $V_a(\tau)$ and $V_C(\tau)$ in the determination of $W_1(p)$ by analogue computer.

The two transfer-functions that were obtained from the correlograms are summarized in Table 1; and in Table 2 the parameters, gain, time delay T, derivative of error coefficient, T_1, relative damping α/ω_0 and natural frequency ω_0 of each of these transfer-functions are listed.

Table 1

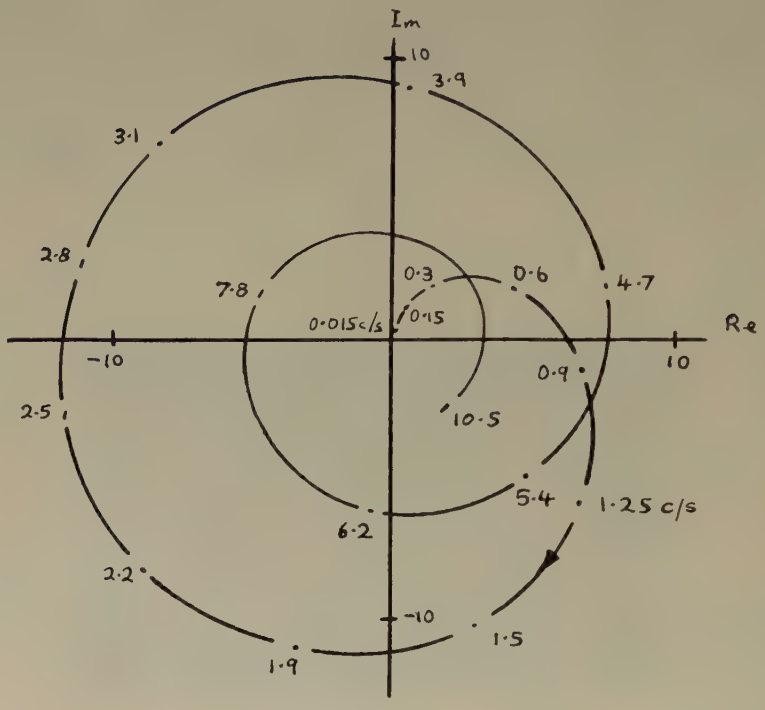
	Case (1) Display gain = 1	Case (2) Display gain = 2
$\frac{\dot{H}}{\epsilon}(p)$	$\frac{64(1+4{\cdot}5p)\exp(-0{\cdot}17p)}{p^2+23{\cdot}2p+240}$	$\frac{17{\cdot}8(1+7{\cdot}1p)\exp(-0{\cdot}15p)}{p^2+24{\cdot}8p+167}$

The frequency-response loci corresponding to these transfer-functions are shown in Figs. 8 (*a*) and 8 (*b*).

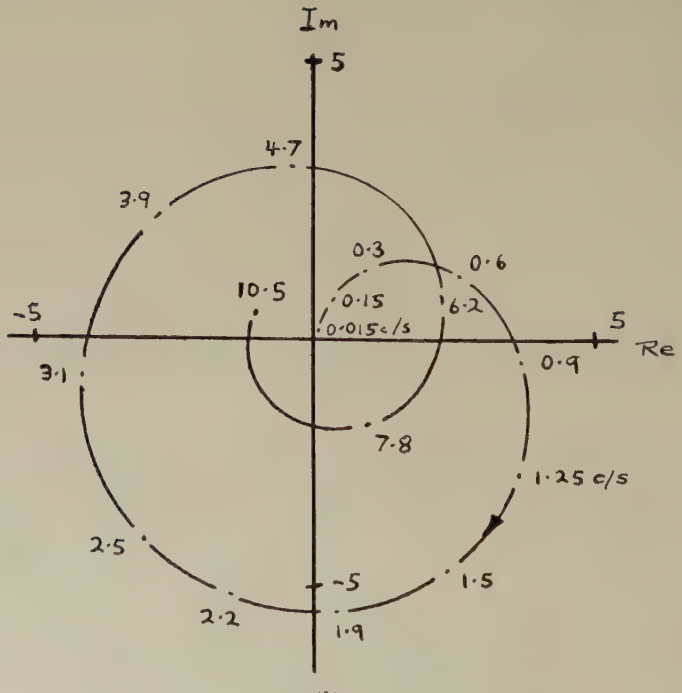
The open-loop transfer-function of the control system is given by the equation

$$Y(p)=\frac{\dot{H}}{\epsilon}(p).A.\frac{2{\cdot}8}{p}$$

where $2{\cdot}8/p$ is the controller transfer function, and A is the display gain, and the corresponding open-loop frequency-response loci have been plotted in Fig. 9. There it can be seen that both transfer-functions give similar loci, and the critical frequency of the closed-loop system is approximately 0·8 c/s in both



(*a*)



(*b*)

Figure 8. (*a*) Frequency-response locus for $(\dot{H}/\epsilon)(j\omega)$ (case 1). (*b*) Frequency-response locus for $(\dot{H}/\epsilon)(j\omega)$ (case 2).

Table 2

Transfer function	$(\dot{H}/\epsilon)(p)$ Case 1	Case 2
Gain	0·27	0·11
Time delay T	0·17	0·15
Coefficient T_1	$4{\cdot}5p$	$7{\cdot}1p$
Relative damping α/ω_0	0·75	1·0
Natural frequency ω_0 rad/sec	15·5	12·9

cases. This is in agreement with results which have been obtained previously from tests with periodic inputs, $\theta_1(t)$. It can also be seen that the margins of stability, as indicated by the points at which the loci cross the negative real axis, are much the same. It is of interest to note that had the operator maintained his gain in case (2) at the same value as in case (1) then the open-loop characteristics for case (2) would have enclosed the point—1 and the system would have been unstable. What the operator has actually done has been to roughly halve his gain so as to maintain almost the same margin of stability. The records for case (2) were taken almost immediately after those for case (1) and this alteration of gain to preserve the margin of stability is yet another example of the highly adaptable behaviour of a human being.

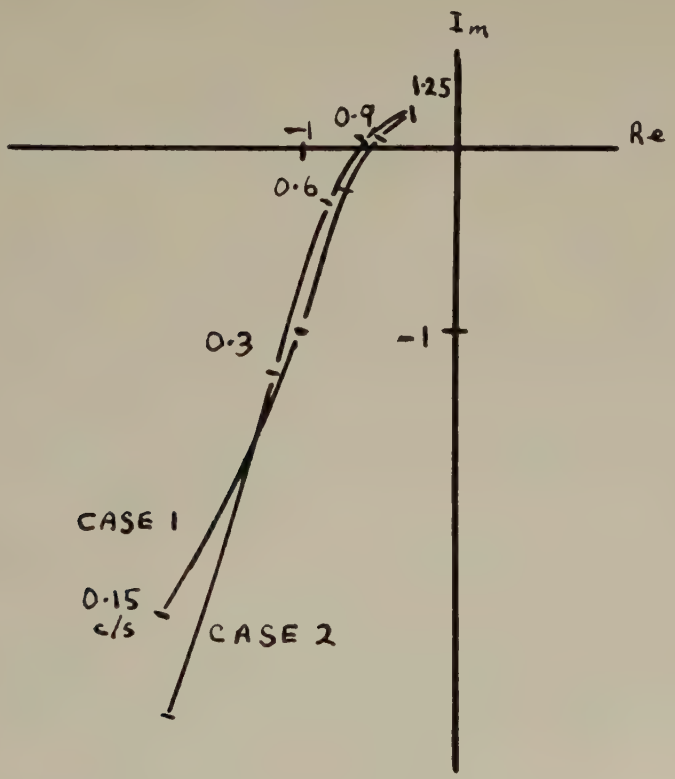


Figure 9. Comparison of open-loop frequency response loci $Y(j\omega)=(2{\cdot}8A/j\omega)\,.\,(\dot{H}/\epsilon)(j\omega)$.

§ 6. Comments on the Results of the Analysis

The interesting features of the results are:

(*a*) The value of the time delay is fairly consistent and its average value is 0·16 sec.

(*b*) The reduction in the operator's gain, which occurs after increasing the display gain, results in a reduction of the natural frequency of his response and is associated with approximately a two-fold increase in the derivative of error term T_1; i.e. the operator tends to give more weight to the derivative of the error when his gain is reduced.

(*c*) The damping associated with the 'motor' system appears to be such as to give an almost critically damped response.

It is concluded that this method of analysis, based on correlation functions, is a practicable one for analysing, in approximate terms and in the form of a previously postulated type of transfer-function, the behaviour of a human operator in manual control situations, and with the increasing availability of digital-computing services and inexpensive analogue computers it is a method well suited for processing the large amount of data that are so easily produced in experiments involving a human operator.

The author is indebted to C. J. Pengilley, B.E., Ph.D., who carried out the experimental work described in the paper.

L'auteur decrit des expériences ayant pour but de trouver, par une méthode d'analyse basée sur les fonctions de corrélation, la fonction de transfert d'une opération humaine lorsque celle-ci fait partie d'un système de réglage fermé.

L'analyse démontre qu'endéans certaines conditions, la fonction de transfert de l'opérateur peut être définie par une équation intégrale qui met en rapport une fonction corrélée transversale de ses réactions à une certaine ' erreur ' à la fonction auto-corrélée de cette erreur.

L'auteur démontre une solution approximative de cette équation intégrale au moyen d'une technique utilisant un calculateur analogue électronique. A ce titre un calculateur de ce genre a été employé pour évaluer les paramètres produisant le meilleur accord entre une fonction de transfert hypothétique et les données expérimentales.

Diese Arbeit beschreibt ein Experiment zur Bestimmung der Transfer-Funktion eines menschlichen Arbeiters, der als ein Element in einem geschlossenen Regelkreis arbeitet, mit Hilfe einer Analysier-Methode, die auf Correlations-Funktionen beruht.

Die theoretische Analyse zeigt, dass unter bestimmten Bedingungen die Transfer-Funktion des Arbeiters durch eine Integral-Gleichung dargestellt werden kann, die eine Kreuz-Correlations-Funktion seines Verhaltens gegenüber einem beobachteten Fehler zu der Eigen-Correlations-Funktion dieses Fehlers in Beziehung setzt.

Es wird gezegt, wie diese Integral-Gleichung annähernd durch eine Technik gelöst werden kann, die einen elektronischen-Analog-Rechner benutzt. Gegenwärtig wird solch ein Rechner benutzt, um die Parameter zu bestimmen, die am besten zur angenommenen Form der Transfer-Funktion der gefundenen Daten passen.

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THE JUDGMENT OF VELOCITY AND PREDICTION OF MOTION

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Experiments are described using an interception situation and varying the amount the subject is able to view of the progress of the object being intercepted. The results indicate that the subjects were able to deal with this type of situation by using a comparatively simple response technique. The main question which remains is the way in which the subject predicts the motion of an object once it has disappeared.

Further experiments were carried out using a prediction situation in which the subject has to press a button when he judges when a moving light which has disappeared has reached a specified point. The results show a systematic relationship between the variability of the subject's performance and the time the light has been invisible. The relationship closely approximates $V^2 = Kt^2$ where V is the variability of the subject, t the time the light was obscured and K a constant. Divergence from this precise relationship appeared to be due to the use by the subject of a well-established response technique.

§ 1. Introduction

There is a vast number of activities which, in everyday language, we would describe as involving the judgment of velocity. The car driver must be able to judge the speed of his own car and that of other road users, while to the pilot the ability to judge speed is regarded as of great importance. Similarly, the judgment of velocity is generally regarded as being of importance in such diverse activities as crossing the road, operating some radar devices, the control of many manufacturing processes and the playing of most ball games. The level of skill acquired by people at these activities varies considerably, but if we take a task such as catching a ball, we find that most people are quite capable of dealing with the apparently complex problem of judging the speed of the ball before it is caught.

Comparatively few investigators have made a study of prediction situations, in which the perceptual data arise from the motion of objects in the environment. Dembitz (1927) included in an extensive series of experiments on velocity judgment a series of experiments using a prediction situation. His moving stimulus was a mark on a continuous belt which could be driven at a variety of constant speeds. The apparatus was concealed by a screen, in which was a 90 cm horizontal slit so arranged that the mark on the belt appeared at one end of the slit, travelled to the other end and disappeared. In the experimental situation the subject was asked to press a key when he estimated the stimulus mark had reached a stationary mark on its path 72 cm beyond the point where its path disappeared. The experiment was performed with the stimulus mark travelling at three different speeds.

The results showed that the greater the speed of the marker, the smaller the error of prediction, or to put it another way, the greater the time to cover unit distance, the larger the error of estimation. The ratio of the error of

* The research described in this paper was undertaken while the author was employed in the Department of the Scientific Adviser, Air Ministry, and was financed through the Flying Personnel Research Committee.

estimation to the speed remained approximately constant, and was analogous to the Weber–Fechner law. From this and other small experiments which included reproducing the speed of a moving belt, judging when one moving point would overtake another, judging when two points moving towards one another would ' collide ', and several investigations of illusions of motion, Dembitz drew the following conclusion. "... the human mind does not follow the physical definition and dimension of the concept of velocity : does not experience velocity as cms/sec ; but rather the reciprocal value sec/cm is valid. Consequently the velocity of movement is not estimated according to the distance travelled in a certain unit of time ; but rather in accordance with the temporal duration required to travel a distance chosen empirically as a unit. Time, duration, thus appears to be the criterion for the mind when comparing motions and their dimensions."

More recently, Johansson (1950) has reported general agreement with Dembitz, particularly with respect to the dominance of the time factor in estimating velocity.

In his own experiments Johansson had the subject compare velocities by the adjustment method until phenomenal equality was obtained. These experiments were primarily concerned with discovering whether the experienced velocity of an object was influenced by the motion of another object in the visual field. His results show that such an influence does occur to the extent that, " two objects in motion in the same field, form if possible, a single configuration of motion or velocity ". However, despite the finding that many physical factors can influence the nature of the configuration, it is his demonstration of the dominance of the temporal component which is most important.

The work of both Dembitz and Johansson involved the subject following the motion of the object with their eyes, as is common in everyday life. Gottsdanker (1952, 1955) used a situation in which the subject had to follow the motion of an object with a pencil and then maintain the motion after the object had disappeared. He used both constant velocity and accelerating objects, and found that the motion of an object travelling at a constant velocity could be accurately maintained for six seconds after the object disappeared. This was not the case with an accelerating object, for which the subject maintained a constant velocity intermediate between the terminal velocity of the object and its average velocity while visible. Gottsdanker interprets this as being due to the integration or smoothing of the foregoing rates which he suggests may be of use when tracking erratic changes in target behaviour.

Knight (1956) has investigated a practical case involving the estimation of velocity in the steel industry. The problem involved stopping steel ingots at a specified point while they were travelling at various speeds. He found that his subjects tended to use spatial clues as ' stopping points ' for the different speeds. This result is of interest, since it gives an example of a skill in which the estimation of velocity is supposed to operate, but for which the subject has provided himself with an alternative technique which gives satisfactory results.

Some mention must be made of the work of Brown (Brown and Baldwin 1953, 1954 a, b, Brown 1955) on velocity discrimination, which was contemporary with the present study. Brown approached the problem of velocity discrimination very much along the lines of Gibson (1954). Firstly, he suggests

that velocity discrimination must be classified according to three speed ranges.

At very slow rates a subject "... discriminates the velocity from successive observations of changes in position. The movement of a minute or hour hand of a watch is not perceived as such but rather inferred from the fact that the hand is seen in different positions at different times".

At faster speeds "... velocities (again) are not discriminated as motion. An observer looking through high powered binoculars sees a fast moving airplane as a blur and needs to perceive the direction and speed of the plane upon the basis of factors such as its sound, expected velocity, and displacement".

At the medium speeds, "... man senses velocity directly as in watching the second hand of a watch, aiming a rifle at a flying duck, or tracking a tank through a gun sight".

Brown's analysis of discrimination at slow rates is a limiting case of the 'signal character' of motion. This is not an unrealistic analysis, since common observation suggests that predictions from such data are almost always made with the aid of some mechanical contrivance. However, his remarks on discrimination at the higher speeds combine the signal and prediction aspects of motion perception. While there is evidence that the character and sound of the object in motion influences the estimation of its velocity (Biel and Brown 1949), the grounds for grouping all 'high speeds' into one class are not compelling.

The experiments were designed to test the hypothesis that at medium speed "... velocity discrimination at optimum speeds (i.e. intermediate speeds) is a single discriminatory event". There is some doubt about the validity of his contention that the results are related to a task in which "... an operator needs to judge the velocity with which it (a target) moves. In order to anticipate the future position of the moving object, he has to know its present direction and speed and very likely higher derivatives of the velocity". The suggestion that the examples of aiming at a duck or tracking a tank support the view that velocity is sensed directly is somewhat questionable. The tracking in both of these tasks surely suggests that the subject is avoiding the estimation of velocity by reducing the skill to a first order task involving only the estimation of displacement or aim off.

A very practical situation is reported by Gibson (1947) who describes the "Estimation of Velocity Test" used by the U.S. Air Force. This required the subject to observe a motion picture of an aircraft flying into a cloud, and then a shell burst somewhere on the screen. The subject had to respond to the shell burst by reporting its position as behind, or ahead of, the now invisible aircraft. Using a 9 by 12 ft screen the subjects could detect a deviation of as little as 1 in. Since this was a group test, with the subjects at widely different distances from the screen, conversion of the distances to visual angle was not practicable. The test presented a strictly limited number of situations since the aircraft always disappeared when half-way across the screen, and secondly only three positions were used for the shell bursts. The limited number of alternative situations, combined with a relatively large number of trials at each condition suggests that the test results may be somewhat misleading as predictors of everyday ability to judge velocity. Gibson in fact reports rather poor validity coefficients against an unspecified criterion.

In summary, much of the earlier work on velocity discrimination and prediction has naturally produced results which are peculiar to the particular situations used for the experiments, but one aspect of the findings does show some consistency. There are various reports of the dominance of the temporal aspect of velocity. The results tend to show that it is variations in the temporal relationship of events which occur as a result of the movement of an object, rather than the velocity of the object as such, which generally produce significant changes in the subject's behaviour. The exact reason for these results has not been shown or suggested by any of the investigators and therefore must be regarded as an important outstanding question.

§ 2. Method

In the experiments to be reported here the subject was seated facing a square screen (20 cm × 20 cm) on to which were projected two small spots of light. One light could move vertically downwards near and parallel to the left-hand edge of the screen ; while the other light could move across the screen from right to left near and parallel to the bottom edge of the screen. The subject was free to move his head, but in the normal position the centre of the screen was level with his eyes, and the vertical and horizontal axes of the screen subtended an angle of approximately 20 degrees.

The task involved adjusting the speed of the horizontal light so that it ' hit ' where their paths intersected. The vertical light moved at a constant speed for each interception, while the speed of the horizontal light was controlled by rotating a small knob operated by the subject's preferred hand. For each interception the lights moved on to the screen simultaneously, the vertical at a pre-determined speed set by the experimenter, and the horizontal at its minimum speed. There thus existed from the moment of starting a potential error, due to the vertical light going faster than the horizontal, which the subject had to reduce to zero before the vertical light reached the point of interception.

The final error was scored either as an undershoot with negative sign, or an overshoot with positive sign. An undershoot occurred when the horizontal light arrived at the point of interception after the vertical, and an overshoot when the horizontal arrived first. In addition, qualitative results were obtained by recording the time/distance relationship of the horizontal light on a kymograph drum.

The apparatus was based on two light sources mounted so that they could be operated independently in elevation and azimuth. The lights were projected from behind the screen and their speed was controlled through a thyratron circuit similar to that described by Carpenter (1952), which operated through a shunt-wound 1/30 h.p. d.c. motor. A full description of the apparatus has been given elsewhere, Gerhard (1957).

The method and apparatus described were used to study the effects of limiting the subject's view of the path of the vertical light. An exploratory experiment was conducted with full view in order to establish the conditions under which the task was reasonably difficult. The main objective was to establish a suitable range of speeds for the vertical light in relation to the maximum and minimum speeds of the subject's light. If the subject had been able to make his light go very fast or very slowly compared with the

vertical light, the task would have been easy. The exploratory experiment made it possible to establish the conditions under which the task required a concentrated effort by the subject in order to obtain approximately 50 per cent error-less interceptions. Two experiments were then made to study the effects of limiting the amount of perceptual data.

§ 3. Experiment 1

In this experiment only half of the track of the vertical light was visible to the subject, as shown in Fig. 1.

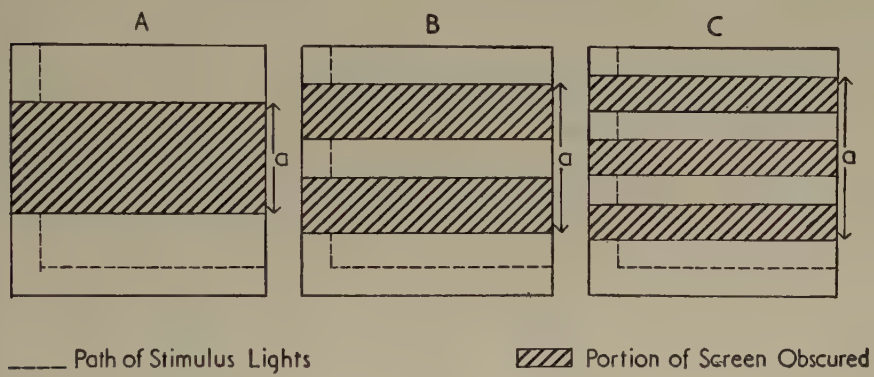

Figure 1

The three conditions of Fig. 1 were presented to the subject in combination with four different speeds of the vertical light. It was not possible to randomize all the twelve conditions during one experimental session, therefore each subject attended three sessions each of which dealt with one of the conditions. In these circumstances, it was necessary to use different orders of presentation of the three conditions for the twelve subjects in the experiment. The subjects, who were all R.A.F. personnel, had eighty trials at each session.

3.1. *Results*

For the lower speeds used in this experiment, it was possible to make a comparison with the results of the exploratory experiment, in which there was full view of the track of the vertical light. The mean errors in the exploratory experiment did not differ significantly from zero, whereas in this experiment the comparable mean errors were all positive, suggesting a tendency to overestimate the speed of the vertical light when part of its path was obscured.

Despite the change in the size of the errors compared with those obtained in the exploratory experiment, they were all small—the longest time error being 0·20 sec, and the largest distance error 7·3 mm. An analysis of the individual results failed to show that the three experimental conditions had any differential effect on the accuracy of performance.

However, the variance of the errors gave some interesting systematic results. In Table 1 these are shown separately for high and low speeds. Apparently, the variability of the subject's performance decreases as the

obscured portions of the track become more evenly distributed (i.e. through the series A–B–C) for the low speeds, and increases for the high speeds.

Table 1

Error in Seconds and Standard Deviation for High and Low Speeds of the Vertical Light

	Experimental condition		
	A	B	C
High speeds	0·23	0·28	0·31
Low speeds	0·34	0·31	0·26

The series of experimental conditions could be extended, increasing the subdivision of the part of the track obscured and thus ' spreading out ' the observation more and more evenly, until the effect would be that of a fine grating covering the whole of the track. According to the results shown in Table 1, the results obtained under the extreme condition of the ' fine grating ', which would occur if the series of conditions A–B–C were extended, should give high variance for high speeds and low variance for low speeds.

In the other extreme case, that of condition A, the higher variance occurs at low speeds. The explanation of this appears to be that with condition A it was the relatively long time the light was obscured which made the task difficult at low speeds. Although the total time the light was obscured was the same for all conditions, with B and C the time it was obscured on any occasion was one-half and one-third of the total respectively. For example, when the light was travelling at its slowest speed, it was obscured once for 4·2 sec with condition A, twice for 2·1 sec with B and three times for 1·4 sec with C.

These results suggest that the subject's variability of performance is related to the time the light is visible and the time it is obscured, which is in turn related to the speed of the light, the amount and the way in which the track is obscured. The results in Table 1 illustrate this interaction between the time obscured and the time seen.

An inspection of the pen trace records showed that the interceptions could be classified into the three types shown in Fig. 2. In those marked X and Y, only two *major* inflections occurred, the first representing the setting by the subject of a speed which was either too fast (X) or too slow (Y) ; and the second representing the final correction in an effort to produce zero error. Interceptions of the type marked Z were characterized by repeated inflections representing successive setting by the subject of his light too fast or too slow. Of all the interceptions, 89·6 per cent were of the types X and Y, and the remaining 10·4 per cent type Z. No significant differences occurred between the three experimental conditions, but as might be expected, the incidence of type Y interceptions tended to be higher with the fast speeds.

The pen trace records show that in the majority of cases, the subject judged the speed of the vertical light before it disappeared, adjusted the speed of his light, and made no further major correction until the vertical light *finally* reappeared. The indication is that, despite the fact that with conditions B and C the vertical light was visible at times between its *initial* disappearance and its *final* reappearance, such ' information ' did not result in major adjustments to the speed of the subject's light.

In summary, the results show that the effective difference to the subjects between conditions A, B and C was the distance between the upper and lower margins of the whole portion of the track partially or wholly blocked off—' a ' in Fig. 1.

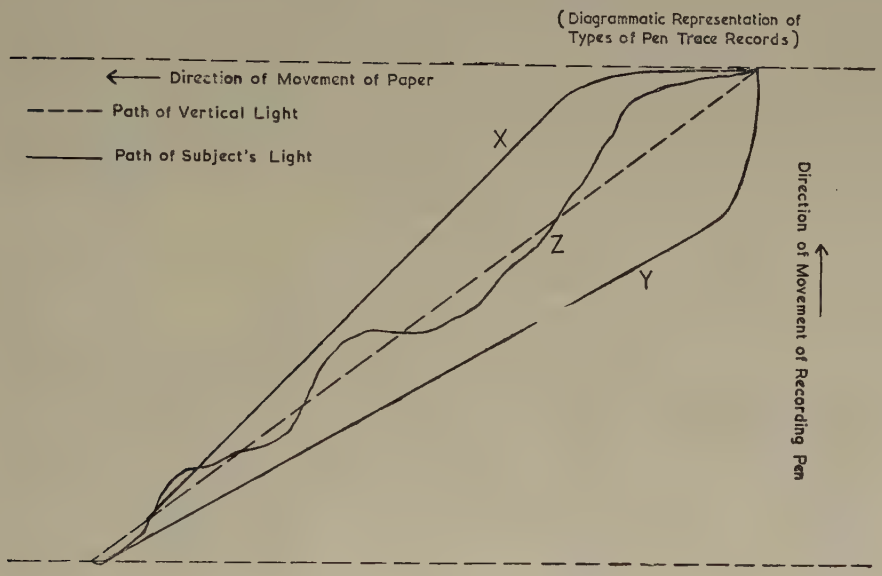

Figure 2

§ 4. Experiment 2

This experiment set out to investigate more closely the relationship between ability to perform the task and the amount of the path of the vertical light obscured. The conditions are shown in Fig. 3. It can be seen that the subject has no opportunity to correct his judgment of when the light would reappear, once it had disappeared behind the obscured part of the track.

Each subject was given two conditions, presented in succession. There were, therefore, a total of twelve paired combinations, which were presented to forty-eight subjects. Each subject had forty interceptions at each condition; twenty were made at the same speed, and the other twenty at higher and lower speeds ; all being presented in a randomized order. The modal speed was 36 mm per sec, which gave a total of 5 sec to complete the 190 mm track.

4.1. *Results*

As in the previous experiment the mean errors were all small and showed little relationship to the experimental conditions. One point which should be mentioned, and to which reference will be made later, is the fact that under condition C the mean error was only 0·73 mm and did not differ significantly from zero.

The variability of the subjects' performance as shown by the standard deviation of their scores in Table 2 showed a steady rise through the series A–B–C–D.

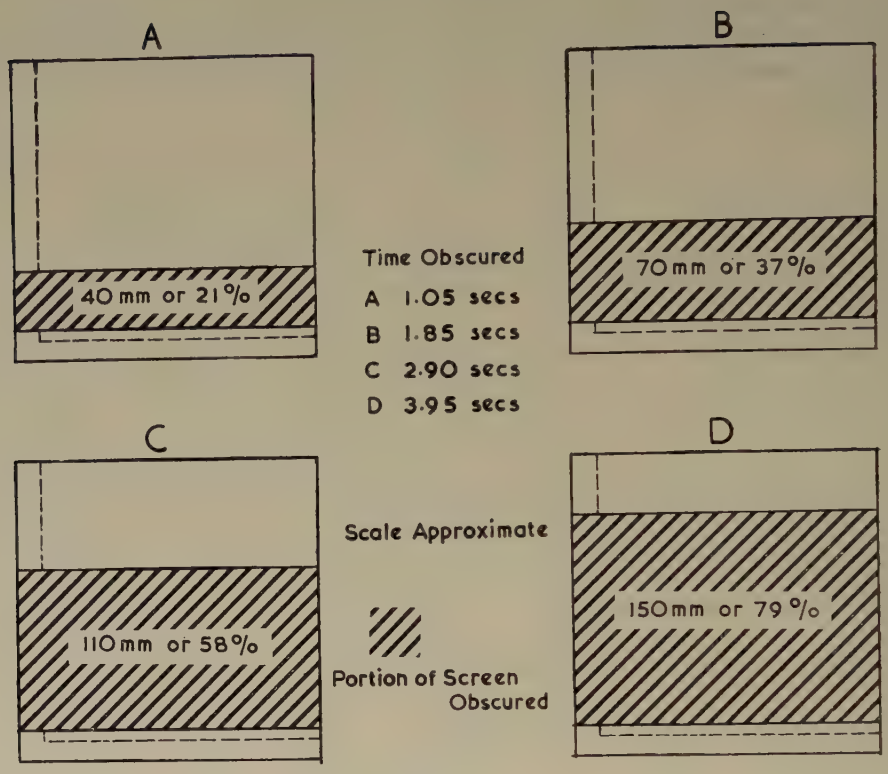

Figure 3

Although no clearly defined technique developed for all subjects, a common procedure, particularly with conditions B, C and D was as follows. As soon as the vertical light disappeared the subject would move his light ' ahead ' of the vertical light, and then turn it down to minimum speed, so that it was near to and slowly approaching the point of interception. Occasionally no

Table 2

Mean Error in Millimetres and Standard Deviation

	Experimental condition			
	A	B	C	D
Mean	2·77	2·53	0·73	3·16
S.D.	11·68	13·67	15·94	16·98

further adjustment would be made, and a ' hit ' would occur. More often though, the subject would either speed up his light, or turn to the experimenter and say that he could not avoid overshooting. This technique bore a marked resemblance to that shown in Experiment 1 with different subjects. The first adjustment of the subject's light seemed to be made on the basis of a rough prediction of the time of arrival of the vertical light at the point of interception. The second adjustment, which was often gross and sudden, seemed to result from the subject feeling that the appearance of the vertical light was imminent. Twenty-six of the forty-eight subjects went so far as to say that once the light had disappeared, they had very little idea where it was until they judged that it was just about to appear from behind the

screening. That is to say, they judged *when* it would appear, rather than where it was.

These statements were not fully supported by the objective evidence. Some 68 per cent of all the interceptions were made with some adjustment to the subject's light after the vertical light had disappeared. From the pen trace records, it was possible to measure the error which would have occurred if no adjustment had been made to the speed of the subject's light after the disappearance of the vertical light. These figures are shown in Table 3, for subjects 1–24 only—there is no reason to believe this to be a biased sample, since a comparison of actual errors for groups composed of subjects 1–24 and subjects 25–48 showed no significant differences.

Table 3

Potential Error in Millimetres at the Instant of Vertical Light Disappearing (Subjects 1–24 only)

	A	B	C	D
Mean	0·82	0·04	−5·64	−1·33
S.D.	10·33	13·25	17·72	14·59

Compared with the figures of Table 2 it is clear that with the exception of condition C the subjects would have done better if they had not altered the speed of their light after the vertical light had disappeared. The fact that such was clearly the case with respect to condition C suggests, as did the actual error scores, that it was 'operationally' very different from the other experimental conditions.

§ 5. Discussion of Experiments 1 and 2

These two experiments were an attempt to observe the behaviour of subjects in a variety of prediction situations, before the subject had had a great deal of practice at the task.

Two aspects of the results appear to be of interest. Firstly, the average errors under all circumstances were relatively small, bearing in mind that with spots of light 3 mm in diameter, an error of 3 mm meant that the spots were touching. Secondly, there is the matter of the response techniques adopted by the subjects. The subjects seldom attempted to adjust the speed of the light early in the interception so that the two lights approached the point of interception at a steady speed and arrived simultaneously. There was instead a common tendency, particularly in the second experiment, to 'get ahead' of the vertical light, wait for it to arrive, and make a last minute adjustment to reduce any error. In this way, the subject appeared to break the task down into two sub-tasks.

(*a*) Judging very approximately when the vertical light would reach the point of interception and adjusting the speed of the horizontal light accordingly.

(*b*) Judging exactly when the vertical light will appear from behind the obscured portion of the screen and making a minor adjustment accordingly.

The subject appears to combine two skills, (i) approximate prediction of when the light will reach the point of interception, and (ii) a fairly exact judgment of when the vertical light will emerge from behind the obscured

portion of the screen. To put it another way, under conditions like those in Experiment 2 the subject is able to judge with considerable accuracy when the light will emerge from behind the obscured portion of the path of the vertical light. However, since he has to 'drive' his light to the point of interception, he must make an approximate prediction of when the vertical light will reach the point of interception so that when he makes his more accurate judgment of when the vertical light will appear from behind the obscured portion of the path, he will only have to make a relatively small adjustment to the speed of his light in order to reduce the error to zero.

The results of these experiments give very little indication of how the subject judges when the vertical light will appear from behind the obscured portion of the screen. The fact that performance under condition C in Experiment 2 was so superior to performance under the other conditions may give a clue in this respect. Under condition C very nearly half of the screen was obscured. Thus, the time the vertical light was visible, and the time it was obscured were approximately equal. In these circumstances, all the subjects would have to do in order to judge when the vertical light would reach the point of interception would be to estimate the time the light was visible and reproduce that time interval. This explanation has the merit of being in accordance with the observations by Dembitz (1927) quoted earlier.

§ 6. Experiment 3

The first two experiments had shown that subjects who were presented with an interception task appeared to make ready use of certain patterns of response techniques which appeared to facilitate the performance of the task. An interception task, with its complex perceptional and motor elements, was used despite the difficulty of neat experimental control in order to make some assessment of those elements of the task which were of critical importance in performing the task.

One such element was the perceptual task of judging the position of an object once it has disappeared from sight, or more specifically the prediction of when a moving object which had disappeared would reappear at a given point. The common everyday example of such a situation is the one which arises when a car is seen approaching a cross-roads and disappears behind a hedge leaving one the task of judging when the car will reach the cross-roads. This and the following experiment were designed to study this process of type of prediction which appears to be of importance in an interception situation.

A horizontal black line was arranged 180 mm from the beginning of the track, leaving 20 mm of screen below it. The vertical light could be switched off at any prescribed point, and the subject's task was to press a button when he estimated the now invisible light had reached the horizontal black line. As the subject pressed the button the light was switched on again giving him knowledge of results.

The subject was shown the point at which the light would disappear on each trial by the position on the track of a similar light which remained visible and stationary throughout each trial.

Six different amounts of the screen were obscured for each trial. These are shown in Table 4 from which it can be seen that these conditions are

grouped in pairs as follows : (a, b) : (c, d) : (e, f). The figures for the ratio time seen/time not seen show that for each simple proportion (e.g. 3 : 1) there was a ' complex ' proportion (e.g. 19 : 3) involving rather less of the screen obscured.

Table 4

Percentage of track obscured

	Experimental condition					
	(a, b)		(c, d)		(e, f)	
Portion of track obscured	15 per cent	25 per cent	40 per cent	50 per cent	65 per cent	75 per cent
Ratio, time seen/time not seen	19 : 3	3 : 1	3 : 2	1 : 1	7 : 13	1 : 3

A design providing these six conditions and five speeds was arranged. It was impractical to provide twenty-five trials at each combination of speed and condition of obscuration, since this would have made the trial too long. It was decided therefore, to divide the entire matrix between three groups of subjects, providing each group with combinations of all five speeds and six conditions of obscuration. The speeds are shown in Table 5. The resulting design was somewhat complex, as consequently was the analysis of the results.

Table 5

Speed of the Vertical Light in mm/sec

Speed				
1	2	3	4	5
42·3	26·3	18·5	13·9	8·8

These aspects of the research are fully described elsewhere, Gerhard (1957). The important point to bear in mind is that it was possible to express the results in two ways. Firstly in terms of the comparison of the performance of individuals (individual comparisons) on the basis of twenty-five attempts by each subject at the experimental conditions presented to him. Secondly, the results could be shown as the average performance of several subjects under the same experimental conditions (pooled comparisons) during the early attempts. The terms ' individual comparisons ' and ' pooled comparisons ' will be used in presenting the results so it is important to remember that the individual comparisons indicate the average level of performance over a relatively large number of trials whereas the ' pooled comparisons ' are based on performance, while each experimental situation remained relatively unpractised.

6.1. *Results*

The mean error scores all approximated closely to zero and showed no significant trends with the experimental conditions. The variance, however, showed a rise through the series a–f and also a rise with the higher speeds of the light.

The relationship of the variability of performance to the experimental conditions for the pooled comparisons is more systematic as can be seen in Fig. 4, which shows the standard deviations against the percentage of the track obscured. The points clearly form a ' family of curves ', each curve representing a different speed of the light. A general rise in the values of the standard deviation with an increase of the time the light was obscured is indicated by two features. Firstly, the curves rise as the percentage of the

screen obscured becomes greater ; for any one speed this is correlated with the time the light was obscured. Secondly, the slower the speed of the light, the higher are the values of the standard deviation. However, if the only factor functionally related to the standard deviation was the time the light was obscured, one would expect the curves of Fig. 4 to approximately be linear or curvilinear, and not ' stepped '.

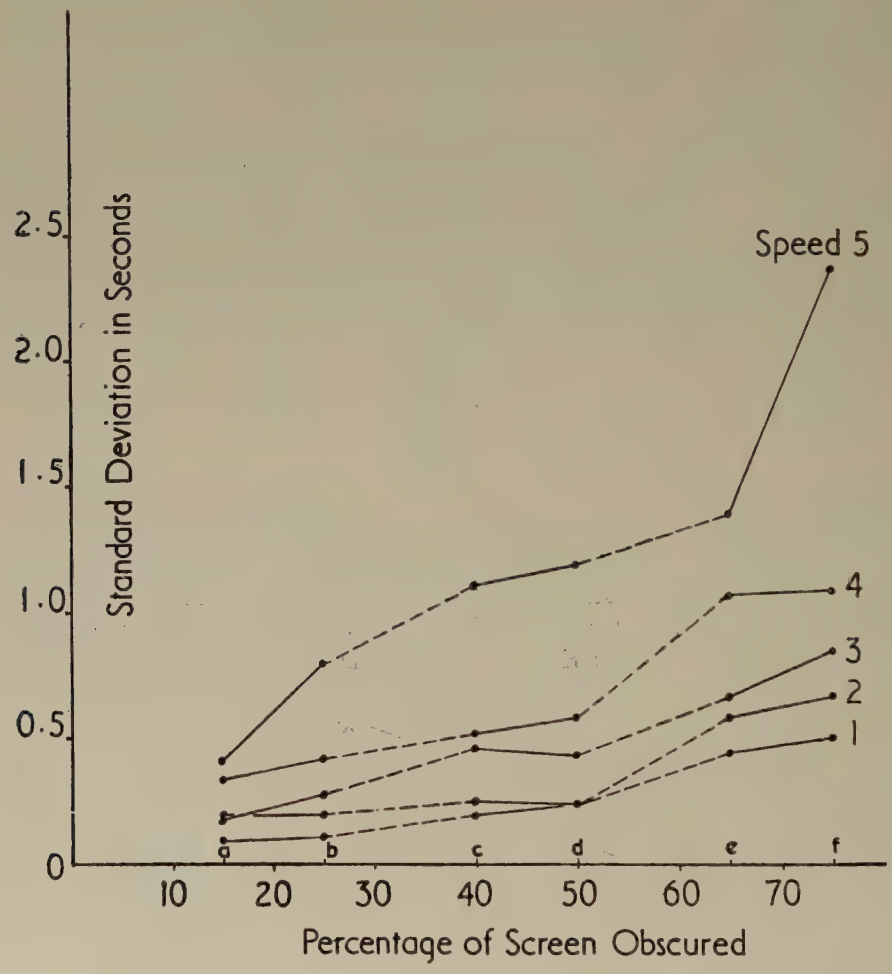

Figure 4

In Fig. 5 the variance has been expressed as a percentage of the time the light was obscured, thus reducing the effect of the time the light was obscured on the general trend of the curves. It can be seen that the curves are even more ' stepped ' than before, and that the undulations of the curves for different speeds follow each other fairly closely. In many cases the variance for conditions (b), (d) and (f) is less than that for (a), (c) and (e) respectively. In addition, it can be seen that the lines joining the following pairs (a, b), (c, d), (e, f), i.e. the full lines, tend to have a more shallow gradient than the (broken) lines joining the other pairs. For example, take speed 1 conditions (b), (c) and (d). The increase of the ordinate value from condition (b) to

condition (c) is relatively greater than from (c) to (d). That is the three points form an inverted 'V ' rather than a straight line or like an ordinary 'V ' like the points (c), (d) and (e) at that speed. A chi-squared test of whether the inflections of the curves in Fig. 5 were due to chance or not gave a value significant at p less than 0·01.

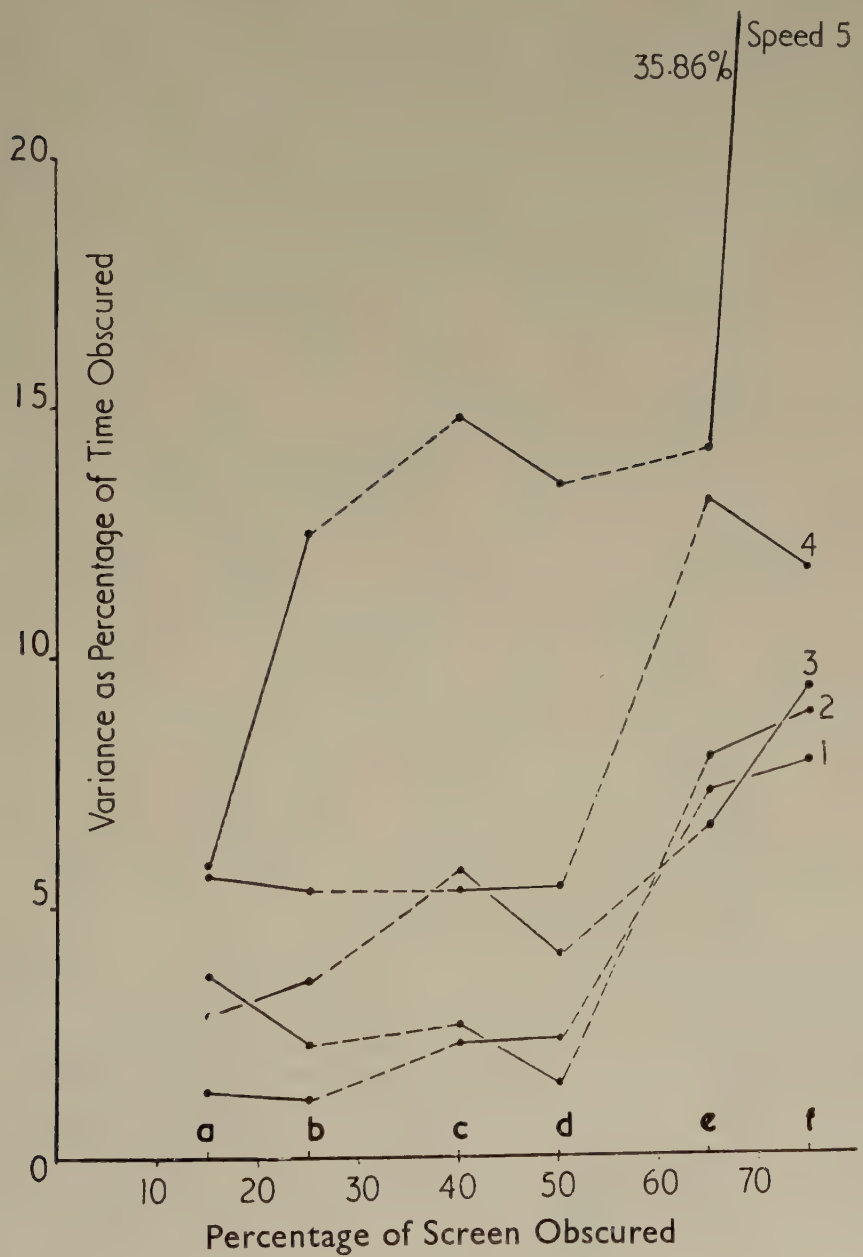

Figure 5

These curves show that despite the general tendency for the variance to rise through the series (a)–(f) there was something about conditions (b), (d)

and (f) which partially counteracted this tendency, which in other words tended to make them relatively easier. It can be seen from Table 4 that these are the conditions for which the ratios, time seen/time not seen, is a simple proportion, and this gives a clue to the following explanation of the results.

Subjects presented with a situation requiring prediction of when a moving object will reach a specified point (the prediction point) when the object has been obscured for some portion of the track proximal to the prediction point, tend to organize the task as follows. During the period the object is visible, the subject attempts to estimate the duration of the period the object will not be visible. The effectiveness of such an estimate is influenced by the extent to which the subject is able to judge the visible part of the track as of equal length, or a simple fraction of the length of the portion of the track obscured.

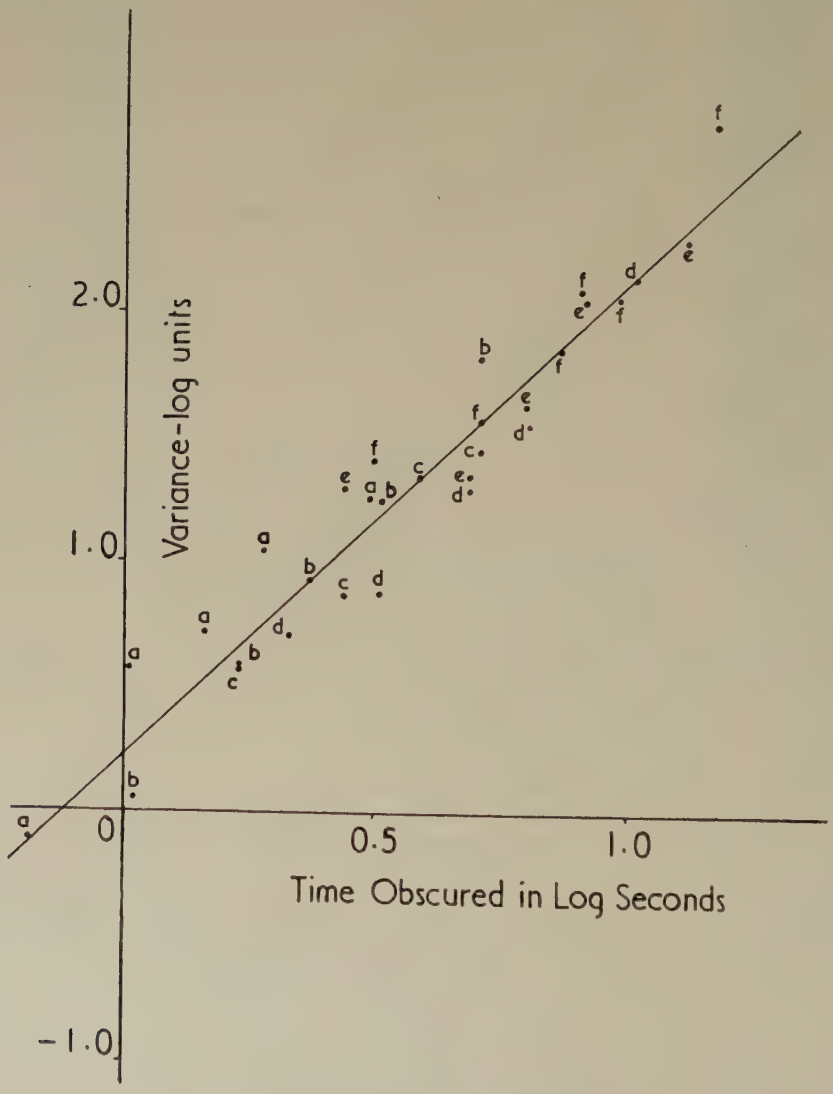

Figure 6

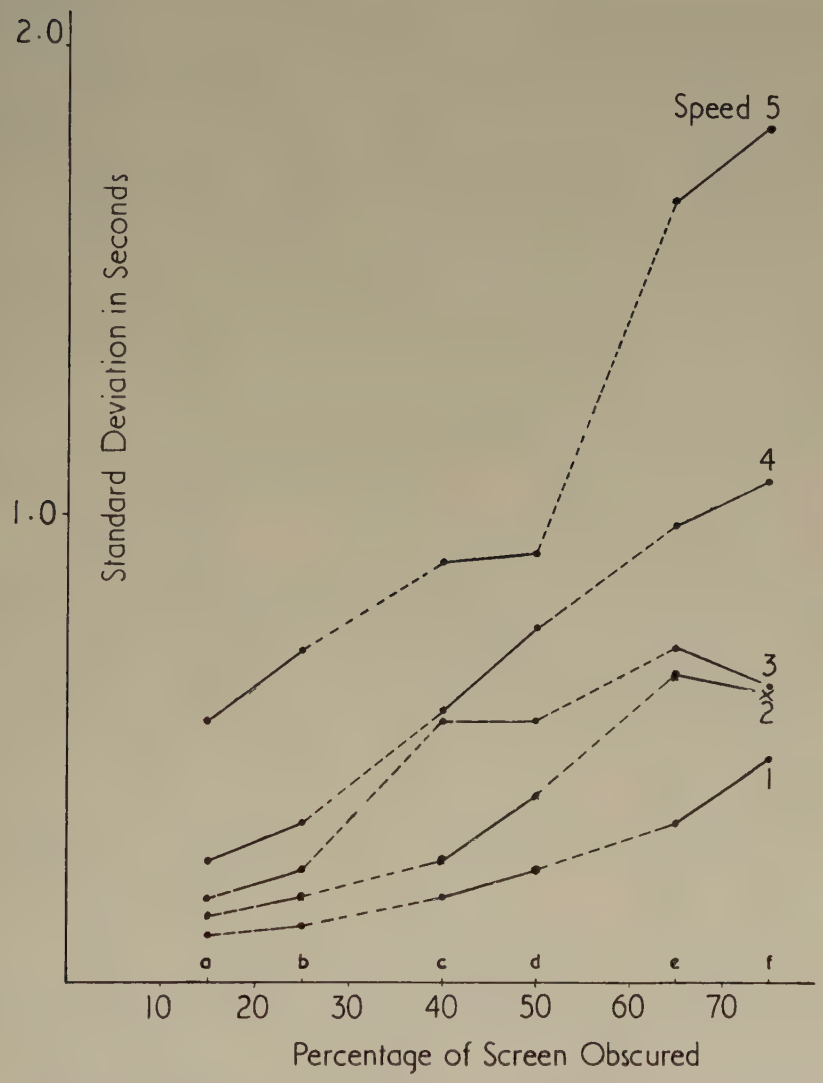

Figure 7. *Note.* The data shown for speed 2 is in a corrected form for the following reasons. In the experimental design each subject had a larger number of trials at speed 2 than at any other speed. For example, one group of subjects had 25 trials at each of conditions (a) and (b) (the individual comparisons) and 10 trials at conditions (c) and (d) (the pooled comparisons) all at speed 2 ; whereas they only had 20 trials at each of the other four speeds. An inspection of the randomized order of the experimental conditions showed that the majority of the trials at speed 2 on the ' pooled comparisons ' came after the majority of the trials at the same speed on the ' individual comparisons '. That is to say, when the subject reached the trials on the ' pooled comparisons ' at speed 2, he had already done some forty trials at the same speed on the conditions for the individual comparisons. Thus the results contributing to the pooled comparisons at speed 2 were not obtained under conditions identical to those at the other speeds. This defect which showed in the results, was rectified by rejecting the original pooled comparisons at speed 2 and substituting the results of the first ten trials of the ' individual comparisons ' at that speed. Further consideration of the results of this experiment will refer to the ' corrected ' results and not to the original figures.

When for example, the visible and obscured portions are of equal length the time the object is obscured and the time it is visible are equal, and therefore the task only involves reproducing a time interval.

In Fig. 6 the variance is shown against the time the light was obscured. Logarithmic scales are used in order to reduce the size of the figure and also to demonstrate the relationship between t and the variance. The points approximate to a straight line, the ' best fit ' is drawn in on the figure. However, it can be seen that the distribution of the points about the ' best fit ' tends to be associated with the experimental condition represented by each point. For example, the points for condition (d) are all lower than the theoretical values. These results suggest that while the variance (S^2) is approximately related to the ' time not seen ' (t) by the equation $\log S^2 = 1{\cdot}91 \log t + \log 1{\cdot}66$,

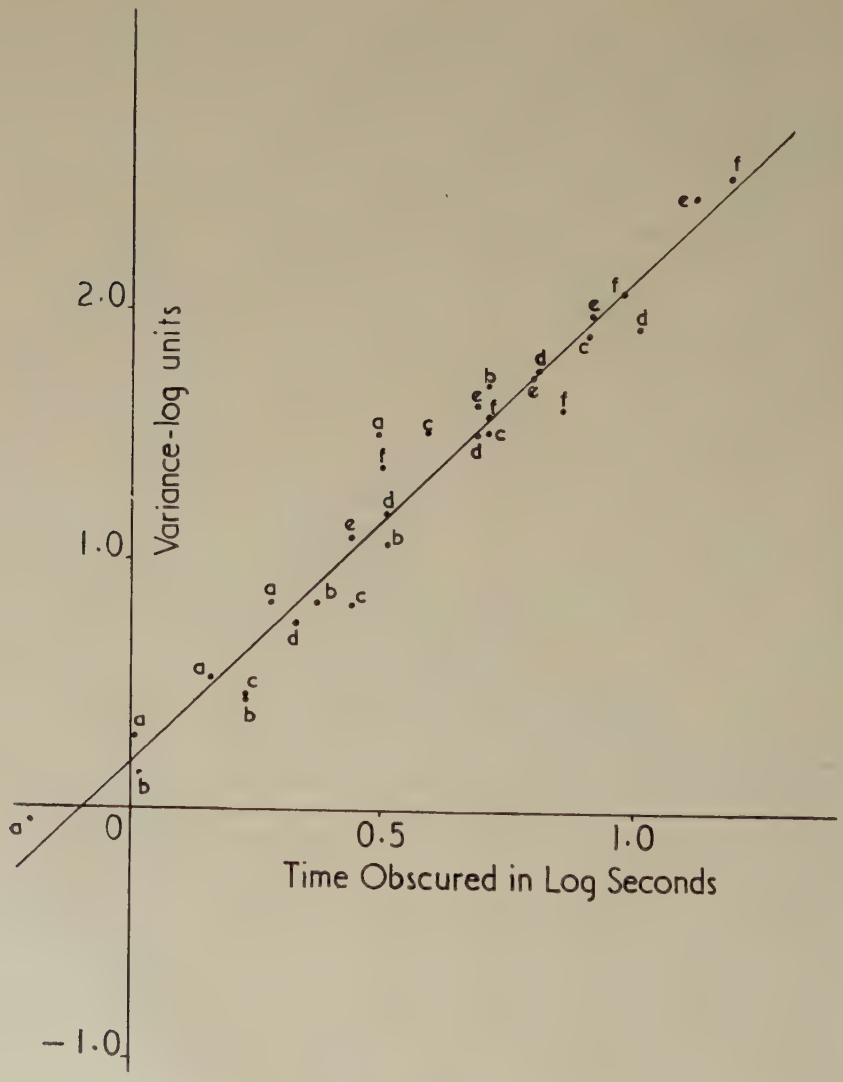

Figure 8

deviations from this precise relationship are related to the ratio, distance seen/distance not seen.

The results presented so far apply to the performance during the first few attempts at each experimental condition. The individual comparisons which were based on considerably more attempts at each experimental condition, gave approximately the same relationship between the variance (S^2) and the 'time not seen' (t) but suggest that performance was not affected by the ratio distance seen/distance not seen.

§ 7. Experiment 4

This experiment differed in two respects from the previous one in that no indication was given as to where the light would disappear. The purpose of the experiment was to throw more light on the reasons for the 'stepped' characteristic of the curves in Figs. 4 and 5, and especially to test the hypothesis formulated to explain this characteristic.

A new group of subjects was used.

7.1. *Results*

Figures 7 and 8 present comparable results to those shown for Experiment 3 in Figs. 4 and 6 respectively. The inconclusive nature of the mean error scores found in Experiment 3 was also typical of this experiment. Individual differences were found, but no consistent trends with the experimental conditions were evident. As with the earlier experiment, the error values were all comparatively small and rarely exceeded the diameter of the spot of light.

The plot of the standard deviations shown in Fig. 7 is similar to those of Experiment 3 in showing a rise of the variability of performance with the time the light was obscured.

§ 8. Discussion of Experiments 3 and 4

The results of these experiments show that errors of prediction tend to be small and the extent of their distribution about the zero error point is closely related to the expression $S^2 = K \,.\, t^2$ where K is a constant and t is the time the light was obscured. It would be pleasant to be able to conclude that this was the exact relationship and that the deviations observed in the present experiments were due to chance factors. However, it is impossible to ignore the evidence that during the earlier trials some of the conditions of obscuration appeared to be unexpectedly easier than the others, seemingly due to the subjects tackling the task by estimating the time the light would be obscured as a fraction or multiple of the time it was visible on the basis of ratio of the lengths of the visible and obscured portions of its path. Without going too far into the field of sheer speculation we can imagine the process when a person is confronted with a prediction situation—a common enough occurrence. He is able with the aid of a relatively well developed ability to reproduce time intervals plus the technique for estimating the time the object will be invisible, to cope without tracking the object with his eyes, integrating the data and making a prediction in a like manner to a radar set, using

comparisons of velocity. For a full discussion of the practical and theoretical implications of this work, the reader is referred elsewhere (Gerhard 1959).

The use of such a technique would be in line with the results described in Experiments 1 and 2 of this paper.

Dans ces expériences, l'auteur a utilisé une tâche d'interception (rencontre de deux mobiles) et il a fait varier la fraction de la trajectoire de l'objet à intercepter que le sujet avait la possibilité d'observer. Les résultats ont indiqué que dans telle situation les sujets usent d'un mode de réponse relativement simple. La principale question qui se posait encore était de savoir comment le sujet prévoit la continuation du mouvement d'un objet une fois qu'il a disparu.

De nouvelles expériences ont eu recours à une situation de prédiction dans laquelle le sujet presse un bouton au moment où il estime qu'une lumière mobile, qui a disparu, a atteint un certain point. Les résultats montrent une relation systématique entre la variabilité de la performance des sujets et la durée de disparition du point lumineux. La relation est très voisine de l'équation $V^2 = Kt^2$, où V exprime la variabilité du sujet, t la durée d'obscurcissement de la lumière, et K une constante. Les écarts par rapport à cette relation précise paraissent dûs à ce que le sujet se sert d'une technique de réponse bien établie.

Experimente werden beschrieben, bei denen eine Versuchsperson ein sich vertikal gleichförmig fortbewegendes Licht mit einem horizontal bewegten steuerbarem Licht zu treffen hatte. Die Ergebnisse zeigen, dass die Versuchsperson fähig war, diese Aufgabe mit einer verhältnismässig einfachen Verfahren stechnik zu lösen. Die Hauptfrage ist, auf welche Weise eine Versuchsperson die Bewegungen eines Objektes vorhersagt, nachdem das Objekt verdeckt ist.

In weiteren Experimenten wurde eine Vorhersage-Situation geschaffen, bei der die Versuchsperson einen Knopf zu drücken hatte, wenn sie der Meinung war, dass ein gleichförmig sich bewegendes Licht, das verdeckt war, einen bestimmten Punkt erreicht hat.

Die Ergebnisse zeigen eine systematische Beziehung zwischen der Variabilität der Leistung der Versuchsperson und der Zeit, die das Licht verdeckt war. Diese Beziehung kommt der Gleichung $V^2 = Kt^2$ sehr nahe, in der V die Variabilität der Leistung der Versuchsperson, t die Zeit, die das Licht verdunkelt war und K eine Konstante ist. Abweichungen von dieser genauen Beziehung scheinen dadurch bedingt zu sein, dass die Versuchsperson eine fest-eingefahrene Verfahrensweise benutzt.

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E.P.A. Project No. 335

"FITTING THE JOB TO THE WORKER"

An international conference on ergonomics was held in March 1959 in Zürich under the above title. The project started in 1956 with a visit to the U.S.A.; the second phase was a technical seminar in Leiden in 1957. The Zürich conference discussed muscular work, workplace layout, display and controls, lighting, noise, climatic conditions and working rhythms. The papers and discussions of the Zürich conference are briefly summarized in the following account. At the end of the conference the employers' and trades unions' representatives presented their conclusions about the application of ergonomics in industry. The wholehearted support for ergonomics expressed in the resolutions they presented clearly justifies the European Productivity Agency in having undertaken this project.—This report was prepared by Dr. W. F. Floyd and Miss I. M. Slade.

UNDER this title an international conference was held recently in Zürich. Organized by the European Productivity Agency, which is a part of the Organization for European Economic Co-operation, the conference was the third phase of a project (E.P.A. project No. 335) which started in 1956 with a visit to the U.S.A. by a group of specialists in ergonomics and a trade unionist drawn from O.E.E.C. member countries. The team spent two months in America, under the guidance of Professor H. S. Belding of Pittsburgh, visiting a considerable number of university research laboratories, industries and other organizations such as those of American Trades Unions, with the object of obtaining first-hand experience of American work in ergonomics. A report of this visit has now been published by the European Productivity Agency under the title "Fitting the job to the worker".

The second phase of the project took the form of a technical seminar which was held in Leiden in March 1957. The 65 participants were mainly physiologists, psychologists, industrial physicians and engineers representing ergonomics in 12 countries. Various international organizations and the trades unions were also represented. The participants included the members of the team which visited the U.S.A. in 1956. The engineers attending the seminar were mostly interested in problems of design, production and work study. The papers covered a great many technical aspects of the problem of fitting the job to the worker.

The Leiden conference (Stansfield 1957) served the purpose of the exchange of technical information between the O.E.E.C. countries, the objective being to enable member countries to obtain up-to-date knowledge of work in the various disciplines represented in the broad field of ergonomics. A resumé of the proceedings of the Leiden seminar has also been published by the European Productivity Agency*.

*"Fitting the Job to the Worker"—European Productivity Agency project No. 335—a survey of American and European research into working conditions in industry. Published for the Organization for European Economic Co-operation by the European Productivity Agency, Paris, 1958.

The purpose of the Zürich conference in March 1959 was to bring together experts in ergonomics and representatives of employers and workers. In this way it was thought that industry could be made more aware of the possibilities of using ergonomics so as to improve working conditions and increase production. Some 200 employers, trade unionists and scientists from fourteen European countries attended.

The papers were read to the conference in general session and simultaneously translated so that they became available in the three languages of the conference, English, French and German. The main work of the conference was done in two subsidiary discussion groups where exchange of information and much useful criticism took place. The topics dealt with at the conference were:

(1) Heavy muscular work, (2) Anatomical factors in work design, (3) The presentation and use of information from the work process, (4) Design of controls, (5) Lighting and noise, (6) Climatic conditions at the work-place, (7) Biological factors in the arrangement of working periods.

The physiology of heavy muscular work is a topic which tends to be highly technical and is often concerned in demonstrating that certain jobs in heavy industry are too heavy as judged by various physiological criteria. The contributions in this field, notably by British, Dutch, German and Scandinavian physiologists, were fully discussed. It was evident from the discussion that there are still many heavy jobs to be done in industry and that mechanization and automation will not remove these heavy jobs except in the larger industries where production is on a very big scale. The two main types of job considered were those in which a large amount of fairly continuous effort is required and those in which there are short periods of intense effort. For the first type of job there appears to be fairly general agreement among physiologists that, on a long-term view, the maximum daily energy expenditure on the job itself should be limited to about 2000 kilocalories (men) and 1500 kilocalories (women) if ill-effects are to be avoided. Various methods of measuring this effort were described and compared. In general much the same conclusions are reached whether the effort is measured directly by the amount of oxygen used, or indirectly by the amount of air breathed, the increase in the pulse rate, or the increase in body temperature or even by other means, such as loss of weight by sweating.

In regard to peak loads, these are often localized to certain muscles and joints, but the evaluation of what can reasonably be permitted is more difficult than in the case of jobs demanding continuous effort. These peak loads can often be reduced by attention to such questions as the use of proper methods of lifting and carrying, frequent changes of position, and improvements in the design of equipment and of the work-place when mechanical aids are not available.

In the session on anatomical factors in work design, consideration was given to the problem of body build and body mechanics in relation to a variety of activities from fine assembly work to heavy lifting. The discussion showed that these anatomical and physiological problems are of widespread occurrence throughout many different industries. Although automation will eventually remove many of these problems there will always remain industries and jobs which will demand a considerable amount of handwork in the nature of assembly line work or assembly of individual units, where only a few items of a particular type are being produced, and in maintenance work.

The discussion emphasized that it is usually impossible to achieve a work-place layout which is optimal in all respects. A compromise has to be effected between the ideal arrangements for different aspects of the work-place, e.g. reach, posture and force. The problem is always a statistical one, and raises the anthropometric question: for what range of the population should the work-place be designed? It was clear from both papers and discussion that a considerable body of knowledge on these problems has been assembled.

The presentation and use of information from work processes was next discussed, ranging from experiments on the design of indicators and on the causes of errors in reading them to the effects of interference, such as background noise and of the value and dangers of established expectations—e.g. when messages are being received. In modern industry a mistake in reading a dial may result in a serious accident or financial loss with waste of materials or damage to plant. Errors can occur three or four times more often with dials and gauges of bad design as compared with those designed on ergonomic principles. The effects of monotony and boredom, also touched on by speakers in previous sessions, were discussed here, especially in relation to repetitive tasks and to such inactive ' activities ' as watching for errors or indications of very infrequent occurrence e.g. inspection tasks.

On the question of the design of controls many contributors to the discussions gave examples of both good and bad forms of control mechanisms found in industry, including tractor pedals and levers, controls on industrial sewing machines, crane controls and simple push-button controls of the type in almost universal use for switching electrical machinery. The papers and discussion on lighting and noise emphasized how much is now known about the former and how comparatively little about the latter, except that noises of 80–90 dB above threshold impair hearing and performance, and if louder can damage the ear permanently.

The last session was given over to a general discussion of the best arrangement of work and rest periods, including some comments on the effects of shift working. Impressive improvements in productivity resulting from frequent short rests rather than occasional long ones were quoted. There is, however, great variation in the practices adopted within the same industry in different parts of the world and even within one country. It becomes difficult, therefore, to reconcile the different procedures and practices which exist. It was clear both from the papers and the discussion on this topic that a great deal of research remains to be done before our knowledge in this field is more than fragmentary.

Throughout the conference the main emphasis was placed by all the speakers on two themes—the great improvements in productivity that had been obtained by consideration of the proper adaptation of jobs to workers, and the improvement in health, comfort and general satisfaction of individual workers that resulted.

Towards the end of the conference, separate meetings were held by the employers and by the trade union representatives to consider what, if anything, should now be done to extend the knowledge and application of ergonomics. The two groups independently produced resolutions which, although differently worded, emphasized almost identical conclusions, as can be seen from the quotations from these important documents given in the following paragraphs.

Dr. Fritz Faubel, on behalf of the industrial representatives present, said

" 1. That the appropriate application in European industry of the ergonomic conceptions demonstrated during this Conference could contribute, not only to the improvement of productivity more particularly in the small and medium-sized enterprises, but also to alleviating undue strain in individual industrial tasks and thereby to the health and well-being of the community;

2. That the conceptions might be applied with advantage not only to the design of machinery and plant intended for the productive processes but also to the design of much manufactured equipment intended for human manipulation;

3. That the Industrial Federations in the various European countries should be invited to recommend their members to take ergonomics principles into account whenever they are faced with problems of the design of machinery and equipment intended for human manipulation;

4. That the European Productivity Agency should be invited to follow up this Conference with such action as will further disseminate the knowledge of past and continuing studies in the ergonomics field."

Mr. L. T. Wright put the worker's point of view. He said that

" The trades unions agreed that it was right that the Conference should have been called, although it has been a scientists' conference and we trades unionists have, for a variety of reasons, done little more than listen in . . . . Much of what we have heard is useful but to us not yet completely convincing."

" We do not see the practical application of ergonomics solely as a tool with which to increase productivity, but as a scientific approach to ease the burden of labour and to be used in the interests of the whole man and not merely in part. We hold the view that ergonomics should be integrated into all work study techniques and should not be considered in isolation, for it can do much good if properly applied. Workers must not be viewed as ' hands ' or ' machines ', and human dignity must never be subordinate to technical efficiency. We believe scientists will pursue their studies in this field as objectively as subjective man is able, but they must recognize that there is likely to be a lot of subjective thought on this matter."

" Where then do we go from here? We believe the trades unions must actively participate in this work. Participation might well start in the national centres, where the services of the scientists might be sought to explain and clarify both to trades unionists and employers. It might be worth considering holding further international conferences separately for trades unionists and employers, with the same scientists present at both. Perhaps the field could be widened to bring in ordinary general practitioners and factory doctors. Here I should like to refer to my own experience as a British trades unionist, when I served as a member of a Committee set up by the British Government, and known as the Committee on Individual Efficiency in Industry. It was set up in 1953 and in 1957 issued its report drawing conclusions from a wide research programme. I should like here to quote from this report in which scientists and trades unionists joined. First, regarding the training of engineers: "

" ' There is a real need to train engineers in some understanding and knowledge of those aspects of anatomy, physiology and psychology relevant to their work (for instance to the design and use of machines), and we recommend the

Department of Scientific and Industrial Research to draw this need to the attention of the responsible authorities in the Government and in industry.' "

" Secondly, regarding psychologists and so on:

' The primary need, however, to which we direct attention, is for persons fully trained in anatomical, physiological and psychological disciplines who can apply this knowledge to the day-to-day activities of industries, and who concern themselves with problems of equipment or plant design, or with management training, or other personnel functions. It is such people, counterparts of technologists trained in engineering or the physical sciences, who should form a vital link in the chain from research in our field to the successful application of its results in industry.' "

" Thirdly, as we are meeting at the Swiss College of Technology:

' The colleges of advanced technology, in our opinion, should also play a major part by establishing senior posts in psychology and physiology, which will enable these subjects and the technologies based on them to grow in intimate association with engineering and other techniques. As well as meeting the need to teach future engineers something of the difference between the characteristics of the human being and the machine, we think that they should train both research workers and technologists who wish to specialize in our field.' "

" If we are to use ergonomics in a useful way, the co-operation of all is most desirable. We shall need a formidable communications exercise, for this lack of clear and adequate communications can prove to be a bottle-neck to all our efforts. We want to continue to learn until we clearly understand. We want to offer our words of encouragement to all working in this field, and, in doing so, to remind ourselves that the machine is for man and not man for the machine."

From this wholehearted support, the conference would appear to have been highly successful in bringing home to those employers and trades unionists present at Zürich the great possibilities of ergonomics. Whether the conference has been successful in a wider sense will depend on the long-term practical results it produces. This will depend ultimately on two things: firstly whether those industrialists present themselves can take action or can convince others of the need to put existing knowledge of ergonomics into practice; and secondly on the availability of specialists to advise on how to do it. This latter may be a limiting factor, since there are only a few ergonomics specialists who have the knowledge and experience required for this task.

Reference

Stansfield, R. G., 1957, Fitting the job to the worker, *Nature, Lond.*, **179**, 1284–1285.

DEPARTMENT OF SCIENTIFIC AND INDUSTRIAL RESEARCH

Ergonomics Sub-Committee of the Human Sciences Committee

Since the formation of the Human Sciences Committee, reported in the May, 1958 issue of *Ergonomics*, a considerable amount of the work of the Committee has been concerned with matters related to ergonomics. As a consequence it has now been decided to set up a D.S.I.R. Ergonomics Sub-Committee. The new Sub-Committee has wide terms of reference and will be concerned with the general development of the subject as well as with the sponsoring of research at universities and elsewhere. So far, the Sub-Committee has been mainly concerned with considering the ways in which the recommendations of the former D.S.I.R./M.R.C. Joint Committee on Individual Efficiency in Industry can be put into effect.

Members of the Ergonomics Sub-Committee are:

Chairman: Professor J. Drever, Professor of Psychology in the University of Edinburgh. (Member of D.S.I.R. Human Sciences Committee.)
Mr. D. E. Broadbent, Director of the M.R.C. Applied Psychology Research Unit, Cambridge.
Professor G. C. Drew, Professor of Psychology at University College, London.
Dr. O. G. Edholm, Head of the Department of Human Physiology, National Institute of Medical Research.
Mr. E. Fletcher, Head of the Production Department, Trades Union Congress. (Member of the D.S.I.R. Human Sciences Committee.)
Dr. A. H. Jones, Medical Officer, British Railways, Southern Region.
Dr. H. Kay, Lecturer in Psychology, University of Oxford.
Mr. W. M. Larke, General Manager (General), Stewarts & Lloyds Ltd.
Mr. A. T. Welford, Fellow of St. John's College, Cambridge and University Lecturer in Experimental Psychology.
Secretary: Dr. D. J. Gerhard.

BOOK REVIEWS

Human Engineering. By E. J. McCormick. New York: McGraw-Hill, 1957. Pp. ix + 467. 60*s.*

A number of books or book-length reports now exist on Human Engineering, under that or one of its other names. Dr. McCormick, who is professor of psychology at Purdue University, agrees with most people in somewhat deprecating the term ' Human Engineering ', but adopts it as the one in commonest use. Among other possible terms, he mentions ' Ergonomics '—a suggestion of which the readers of this Journal will doubtless approve, although, strictly, the latter term covers a wider field than the former.

The author's stated intention is the undeniably difficult one of steering between the Scylla of a mere compilation of condensed research data (in what he calls " the typically pedantic language of the experimenters ") and the Charybdis of a set of " cookbook recommendations ". Most writers on this (and on certain other) subjects seem anxious to file the same claim to having, at least, meant well. But if neither research data nor ' do it yourself ' instructions appeal to the author as targets for his particular ammunition, it is fair to ask what is the third target which appeared more profitable than either. Ideally, it should be to enable the reader to deduce solutions to his particular problems—to present, in other words, ' the theory ' of the subject, even if the theory must, at present, consist largely of working hypotheses eked out by empirical rules.

Considering the nature of the subject, to come anywhere near fulfilling such an ideal would demand exceptional qualities in both author and readers, for the result would inevitably be classed as highly technical and difficult. The publisher's assessment of the market for books on Human Engineering is signified clearly enough by the statement on the dust-cover that Dr. McCormick's book is ' non-technical '. How an ostensibly serious, if introductory and elementary, work on Human Engineering could be described as non-technical passes my comprehension. Admittedly, it is far too longwinded and inflated with padding—e.g. " It is very handy to be able to know where we are in relation to our environment . . . "—but a considerable proportion of it is unquestionably technical. On the whole, the technical matter is at a schoolboy level, but without a good school textbook's careful and accurate exposition.

These are serious criticisms, and are not made lightly. Human Engineering is very far from being a straightforward technology; neither is it so securely established or so unimportant that a potboiler on it can be regarded with gentle tolerance. On the other side of the account, it is difficult to think of any appreciably better introduction to almost the whole vast field (the omission of manual-control dynamics is in keeping with the general intention). The author has listed a wide variety of references and has summarized many interesting experiments. If the " engineers and managerial personnel " for whom he has primarily written merely want an impression of what Human Engineering, as now practised in America, is about, they may well find the book is to their taste.

W. E. Hick

The Time-span of Discretion in Job Analysis. By J. M. M. Hill. Tavistock Pamphlets No. 1. London: Tavistock Publications. A reprint of an article in *Human Relations,* Vol. IX, No. 3. Pp. 29. 3*s.* 6*d.*

The author, who is Policy Research Officer in the Glacier Metal Company, describes the methods he used and the difficulties he encountered in carrying out a special job analysis programme over a period of eighteen months. The methods used were based on the concepts put forward by Dr. Elliot Jaques in *Measurement of Responsibility* (London: Tavistock Publication, 1956). Briefly, the idea is that remuneration can be related to the length of time a person performs a job before his immediate senior needs to review the work from the aspect of the discretion that the person has been expected to use. For manual work in a factory, the time-span can be found, usually, by determining how long it is after an operator has been ' indiscreet ' before the ' line ' stops !

Whilst for staff jobs the idea may have some merit, it is doubtful whether it will have much impact on the remuneration problems of our industrial society. Mr. Hill's article should, however, be read by the specialist concerned with problems of remuneration and interested in a new technique.

The major problem with data about individual jobs in a company, particularly with regard to remuneration, is that because of the confidential nature of the details it is difficult to discuss them in public. Mr. Hill has used as examples ' composite ' or ' disguised ' jobs in order to overcome this problem.

ERG.

For those who feel they would only be prepared to face the 'time-span' involved in carrying out Mr. Hill's type of analysis if there were a reasonable expectation that time so spent would be justified by the results, the article may whet their appetite for a follow-up article in which time-span and salary are related.

Specialist staff who have carried out job analysis and/or job evaluation will sympathize with Mr. Hill over the difficulties he found when asking questions of managers about jobs under their control. It is possible that these difficulties arise from the defensive or offensive(!) reaction to enquiries about the workings of a department. Whilst it is easy to ask questions it is not always easy to get simple answers which, after subsequent discussion, can stand unqualified. In addition, particularly when the emphasis is on discretion, there is the problem of differentiating between the basic amount of discretion implied in the nature of the job, and the discretion or judgment exercised by the people doing the job. Many staff jobs do change, particularly over several years, as a result of the abilities of the people on them.

Mr. Hill gives examples of time-span analysis for a draughtsman, a machine operator and a research worker. Whilst the article contains definitions and detailed explanations of many of the facets of this particular approach to analysing jobs in accordance with Dr. Jaques' concepts, it remains to be seen whether many other specialists in industry will try to prove Dr. Jaques' hypothesis. As Mr. Hill says, " others must be able to check Dr. Jaques' findings by their own observations and experiment and for this they must be able to discover the time-span of discretion of jobs. Until this happens the hypothesis is untestable and the practical value of the notions put forward cannot be adequately assessed ". There is little doubt that any company that carries out such a job analysis programme will learn a lot more about its own organization and, apart from the usefulness for wage and salary structures, the company should find the survey valuable for improving its efficiency.

L. V. Green

Yearbook of International Organizations (Annuaire des Organizations Internationals). 7th edition, 1958–59. Union of International Associations. Publication No. 160. Palais d'Egmont, Brussels, Belgium, 1958. Crown octavo, pp. 1269. $14 (U.S.), 78*s.* (sterling), 580 (Belgian francs), 5800 (French francs), 7250 lire, 52 (Swiss francs), 52 DM.

In 1945, when the new era of the United Nations began, it was felt that, with the growth of international organizations, a directory was necessary, to enable responsible persons everywhere to obtain authoritative information concerning these new instruments of international action. The UIA (Union of International Associations), which was established in 1907, unwittingly came to the rescue, by publishing its *Yearbook of International Organizations*, in 1950, about the time when UNESCO decided that a directory was needed. It was realized that this yearbook filled the gap and it was proposed that the publication should be entrusted to UIA. It has since carried out this important task since 1950 at no expense to the organizations.

After a brief preface, introduction and explanatory notes, which should be read by all who wish to make the most of the Yearbook, there follows a table of the contents which are divided into six sections.

The first five sections contain entries regarding 1209 international organizations (149 governmental and 1060 non-governmental) and the names of 8423 of their officers. Each entry gives a brief history of the organization, its officers and legal status, structure, budget, relations with other organizations, activities, publications, etc. The whole of this information has been condensed into 1101 pages. There are, in a further 16 pages, similar entries concerning 13 national non-governmental organizations, in consultative status with the United Nations and a list of Institutes of International Affairs.

The sixth section comprises various indexes: subject indexes in English or in French; index of initials; geographical index to addresses; French glossary; and a list of organizations appearing in this Yearbook but not in the 1956/57 issue.

This edition of the Yearbook, is a fascinating compilation even for those who have no personal interest in any international association, and one is astounded at the wealth of information contained in an average of a little more than a page to each organization. Especially interesting is a 16-page account of the United Nations Organization.

" Purely as a matter of interest, to find out if the Yearbook would be of use to one who was wanting information on any matter of recent interest, the question of fishing in the Mediterranean was looked up. The English Index reference " Fisheries Council for the Mediterranean, General " gave reference to Entry No. 59 " General Fisheries Council for the Mediterranean ", which stated that the Council was established 20 February 1952, that 12 Governments (named) were members, that the aims were to identify fisheries problems in the Mediterranean and Middle East, to seek

solutions and when appropriate to encourage research, to exchange and disseminate information, to encourage and organize training centres on subjects related to fisheries. The names of the Officers of the Council were given. The Council was financed by a grant from FAO (Food & Agriculture Organization as reference to the index of initials discovered) and Expenses for 1957, excluding salaries came to $4,750. The activities carried on by the Executive Committee and their five technical committees included (1) Exploration dealing with certain fish and trawling grounds: (2) Work on methods and equipment, training of fishermen etc. in the Mediterranean: (3) Utilization of catch; (4) Problems of Inland Waters: (5) Economics and Statistics. There were meetings in Istanbul (1956) and in Rome (1958). Proceedings and technical papers are published after each meeting, also an information bulletin, studies and reviews.

This account, chosen quite at random, would obviously put anyone who was interested in tuna fishing or sardine fishing in the Mediterranean ' in the picture ' ! "

The statement is made on p. 14 of the Explanatory Notes: " The Yearbook is not intended to be a work of literary art ". It is an attempt " to present more and more facts in less and less space ". After looking up dozens of subjects and following out random trails I have been quite overcome by the sheer beauty of the work. In my opinion it *is* a work of art, which must have been a source of great gratification to Mr. E. S. Tew, Administrative Secretary of the UIA, and to the numerous workers of all grades who have helped in its production. It will give satisfaction also to those into whose hands it comes—and I hope that these latter will not all be referring to it for purely utilitarian reasons. I would suggest that the penultimate paragraph of Mr. Attwood's Foreword (p. 9) be modified to read: " Every embassy, foundation, research council, library, and agency of Government concerned with international affairs should have at least one copy of the Yearbook for ready reference and a second copy for leisure moments ".

The printing and general layout are of a high standard but I would prefer other type faces, e.g. such as are used in this journal.

H. Banister

The Church and Industry. London: The British Council of Churches, 1958. Pp. 40. 1*s*.

A working party of the Social Responsibility Department of the British Council of Churches are the authors of this discursive essay on an aspect of human relations. They write with sense, breadth of sympathy, modesty and—most wisely—a careful avoidance of ' Church jargon '. Certainly no reader can quarrel with their main contention, which is that the complex structure of modern industry demands of the religious person, ordained or lay, who seeks to be helpful and influential for good, not only personal qualities of a high order, but also a great deal of special knowledge. The authors urge that the necessary instruction and organization should be planned on a national basis. They conclude with the intriguing remark: " . . . if the Churches really care . . . about the ' Church and industry ' had they not better start by paying for any gesture they propose to make ? "

W. E. Hick

Arbeitswissenschaft. By H. H. Hilf. München: Carl Hanser Verlag, 1957. Pp. 341. Illustrated.

One of the difficulties when reviewing a book on ' Arbeitswissenschaft ' is that there appears to be no strict English equivalent of ' Arbeit ' and ' Arbeiter '. To me, ' Arbeiter ' means a man engaged in an essentially manual task, not just any worker: artisan would perhaps be a better term. Nor does ' Arbeit ' seem to be adequately translated by either ' work ' or ' labour '.

Professor Hilf has brought together in the confines of one volume material which covers the whole field of ' Arbeitswissenschaft ', i.e. the science of work. The book is essentially intended for ' practical ' men who are directly responsible for the employment of labour. It is less attractive to the student or research worker because it largely restricts itself to didactic statements without giving any supporting evidence; this would indeed be impossible in a book of the present scope.

It is divided into seven sections: Section I deals with the background and history, and reviews the instrumentation which has been used in work study and gives some bibliographical pointers. This is one of the most valuable and readable sections of the book. In Section II the context in which work is performed is examined. The discussion does not restrict itself to the physical environment, but also considers anthropological, ethical and social influences. Section III, entitled " The Biology of Work ", includes industrial physiology, industrial hygiene, industrial psychology and training methods. In view of the fact that the majority of the readers will be primarily concerned with production, Professor Hilf rightly emphasizes in the physiological chapter the danger that rationalization of the work process in terms of time required per operation can lead to physiological overload of the worker in terms of energy expenditure.

The next section deals with the work process itself. It contains chapters on suiting the tool or the machine to the worker, and on time and motion study. One minor fault, which applies to the whole book, is more obvious in this section than in others. There are too many diagrams, some of them so complex that they irritate the reader rather than elucidate the subject matter, while a few merely illustrate a turn of phrase. Sections V and VI deal respectively with the organization and planning of the industrial process and the economic aspect of work, both in relation to the payment of the worker and the cost to the employer. Considerable national differences no doubt exist in the reaction of the working population to different schemes of payment, and I think that it should be borne in mind that Professor Hilf's conclusions may not be valid in other countries.

The last section discusses the resistance which the ' work scientist ' is likely to encounter from the worker himself in his efforts to apply his techniques, and warns against the introduction of individual changes which are out of context. The book ends with an examination of the place of work science in the world today and suggests how a work scientist might be trained. An appendix contains an individual bibliography for each section. There is no doubt that Professor Hilf has done a major service to the cause of work science in shouldering the prodigious labour of assembling the information necessary for a work of such wide scope. The book is very well printed and pleasant to handle.

H. S. Wolff

ERGONOMICS RESEARCH SOCIETY

ANNUAL GENERAL MEETING

The 10th Annual General Meeting of the Society was held in Balliol College, Oxford, on Wednesday, 8th April, 1959. There were 60 members present. Dr. J. S. Weiner took the Chair.

After the Minutes had been signed, the Hon. Secretary (Dr. O. G. Edholm) and the Hon. Treasurer (Mr. K. F. H. Murrell) presented their reports, which were approved, and Mr. Welford gave a verbal report on the journal *Ergonomics*. He said the journal had had a most satisfactory welcome from all over the world and was now circulating very extensively. Dr. Floyd had been appointed Associate Editor and was dealing with all the physiological papers.

In discussion, Dr. Franklin Taylor (U.S.A.) spoke of the difficulties experienced by those in America who had wished to start a similar journal, and added that *Ergonomics* was filling a very important need for workers in this field in the United States.

The next business was to consider a list of amendments to the Rules. Dr. Edholm, in presenting these to the meeting, said that the reason for the changes was that the Society found it was unable to continue to function without an increase in the number of its Officers. The work of the Society was becoming steadily heavier and, with the growth of its importance, it was likely that there would be an acceleration in business. The Council, therefore, recommended that the Society should have the power to appoint up to five Secretaries which would enable the work to be divided. The changes proposed in the Rules gave such powers, and also indicated the duties of the various Secretaries.

After discussion, these amendments were approved by the meeting.

Election of Officers

The names of Officers and Members of the Council given below were approved, including those of Dr. Hellon and Dr. Griew to fill two of the newly created positions.

Chairman of Council
T. Bedford

Hon. General Secretary
O. G. Edholm

Hon. General Treasurer
K. F. H. Murrell

Hon. Membership Secretary
R. F. Hellon

Hon. Conference Secretary
S. Griew

Members of Council

J. E. Cotes
E. R. F. W. Crossman
L. V. Green
R. G. Hopkinson
Miss A. D. K. Peters
Miss I. M. Slade
R. G. Stansfield
D. Wallis

There was general support from the meeting to the proposal that there should be overseas representation, and it was agreed to refer this problem to the Council.

Membership of the Society

It was agreed that the number of Ordinary Members be raised from 200–250. The Secretary said that since it had been agreed that there should be an increase in the Officers of the Society it should be possible to deal with a larger membership. There had been many well-qualified applicants whom the Council considered would be valuable members but they could not be accepted until the number of members was increased to over 200.

Election of Members

The following were elected Members :

	Proposer	*Seconder*
A. W. Box (London)	Reginald Sell	I. M. Slade
J. Brozek (Bethleham, Penn., U.S.A.)	R. MacFarland	O. G. Edholm
P. Cavanagh (London)	D. Wallis	H. G. Maule
W. P. Colquhoun (Cambridge)	D. E. Broadbent	R. Conrad
F. A. Chrenko (London)	T. Bedford	A. D. K. Peters
P. de Coulon (Neuchatel, Switzerland)	E. Grandjean	O. G. Edholm
Dean Farnsworth (U.S.A., London)	H. Murrell	O. G. Edholm
P. W. Humphreys (Oxford)	A. R. Lind	J. S. Weiner
S. Laner (London)	I. M. Slade	R. G. Sell
D. Lecoultre (Paris)	A. D. K. Peters	J. E. Cotes
C. S. Leithead (Liverpool)	G. Crowden	J. R. Brown
M. C. W. Long (London)	W. F. Floyd	O. G. Edholm
L. G. Norman (London)	W. F. Floyd	O. G. Edholm
E. F. O'Doherty (Dublin)	H. Murrell	O. G. Edholm
M. Powell (London)	G. P. Crowden	H. G. Maule
J. D. Redmill (London)	A. D. K. Peters	C. C. Molloy
Alec Rodger (London)	D. Wallis	H. G. Maule
J. Spencer (Farnborough, Hants.)	H. Murrell	E. R. F. W. Crossman
H. B. Wright (London)	C. Lovatt Evans	W. F. Floyd

European Productivity Agency Project on " Fitting the Job to the Worker "

Miss Slade gave an account of the meeting in Zürich, which is the subject of a report in this number of the Journal.

Mr. Stansfield supplemented Miss Slade's account and said that there was no doubt that the Zürich conference had been valuable in making a very large number of organizations aware of the existence of Ergonomics. The conference had received wide publicity and the effects of it would continue for a long time.

Professor Bürger gave an account of the developments that had taken place since the conference in Leiden in 1957, under the auspices of the European Productivity Agency. At that conference it had been decided to set up a Committee to examine the desirability of an international association, particularly for the holding of international meetings.

Professor Bürger's account was followed by a useful discussion, and the Secretary gave it as his opinion that the establishment of an international association would greatly strengthen the essential work of the Society, which was to encourage research in the field of ergonomics. He hoped for his part that the members present would support such a development.

Other Business

Professor Müeller said that he had greatly enjoyed the meeting at Oxford but he wondered if the Society was not over-emphasizing the educational aspect. At this meeting at any rate the majority of the papers had been either reviews or of a fairly elementary character, and he

hoped it was not the intention of the Society to discourage the giving of research papers. Owing to the late hour, it was not possible to discuss Professor Müeller's important point in detail, but he was reassured that it was not the policy of the Society to concentrate its activities on educational work but that the present meeting had been specifically designed to indicate the industrial applications of Ergonomics.

Before the meeting closed, the Secretary expressed, on behalf of the Society, his thanks to the Master of Balliol and to his colleagues who had made their stay at the College such a delightful experience.

The meeting then closed.

PROCEEDINGS

A Meeting was held on 9th January, 1959 at the Medical Research Council Laboratories, Holly Hill, Hampstead, London, N.W.3.

The following papers and demonstrations were presented :—

Papers :

(1) " Some Researches into Thermal Comfort ", by T. Bedford.

(2) " Subjective Reactions to Infra-red Radiation ", by F. A. Chrenko.

The responses of the human body to changes in environmental thermal radiation are of interest to the physiologist and psychologist who investigate mechanisms of heat regulation and perception of external stimuli, and not least to architects and engineers concerned with designing and maintaining comfortable and pleasant working and domestic environments.

Knowledge of the manner in which sensations of warmth are evoked by changes in the external thermal environment was made more precise when experiments were conducted, especially by Hardy and his collaborators, with thermal radiation as the stimulus. This was an improvement on previous methods which involved tactile and thermal stimuli simultaneously. Hardy *et al.* measured the smallest intensity of thermal radiation which would evoke a sensation of warmth on the forehead of twelve ' white ' subjects who showed " moderate differences of sensitivity ". Sensitivity is generally measured in terms of a ' threshold ' which represents the point of transition between the stimulus which is insufficient to evoke a sensation and one intense enough to do so, and in the classical ' method of limits ' the stimulus is first increased stepwise until the sensation is evoked and then decreased until the sensation ceases.

We conducted experiments at Hampstead in order to study, among other things, the variation of sensitivity to radiative stimuli amongst several subjects, the majority of whom were not specially trained, and our choice of the method of limits was endorsed by a psychologist's view that this was the only psycho-physical method sufficiently adaptable for rapid and efficient tests on a large number of untrained subjects. Since our group also contained experienced subjects, it was hoped that the results would provide information of use not only to the scientist but also to the architect and engineer. The latter, in any event, has to deal with ' untrained ' subjects.

Although subjective reactions to radiative stimuli have been described as bewildering in their variety, we attempted to minimize this by (1) irradiating the skin of the forehead only, (2) keeping the area constant at 40 cm^2, and (3) always giving an exposure of 3 sec duration. By repeating experiments on 50 subjects until we obtained over 300 thresholds, we were able to obtain quantitative information concerning the sensitivity of our subjects.

The type of radiation used in these experiments was initially ' non-penetrating ' infra-red with a wave length of maximum intensity of about 3·5 microns emitted from a full radiator similar to one designed by the British Iron and Steel Research Association.

Preliminary analysis of the results indicates that the logarithms of the thresholds are distributed in a statistically normal manner with a mean of 6·2 mw/cm^3, and the observations range from about 2·5 mw/cm^2 to 12 mw/cm^2. Hardy's subjects were thus as sensitive as our most sensitive subjects, but about four times as sensitive as our ' average ' subjects.

There also appears to be an inverse correlation between threshold and skin temperature, that is, as the skin temperature increases the threshold decreases. In other words, the sensitivity of the subject increases with a rise in skin temperature.

Other experiments have indicated that as the temperature of the source of radiation rises, the threshold rises, that is to say as the wavelength (λ max) of radiation decreases, so does the sensitivity. These results are in accordance with practical experience, and they also provide a physiological basis for the well-known diversity of subjective reactions of groups of people to a given thermal environment.

(3) " Studies of Comfort Sensations in Antarctica ", by J. M. Adam.

(4) " The Assessment of Solar Radiation ", by C. R. Underwood.

One of the factors which affects man's performance outdoors is the energy he receives from the sun in the form of radiation. It is of interest to the physiologist to study the effects of this radiation on the body, but before he can do so he must have some idea of the magnitude and nature of the energies involved.

The question which appears to demand an answer may perhaps be stated in the following terms : If a man is exposed outdoors anywhere on the earth, at any time of the day, what is the rate at which he receives energy from the sun and what is the nature of that energy ?

It is fundamental to our problem to have some knowledge of the sun as a radiator and this is, and has been, the subject of much study. Observation has always been hampered by the presence of the atmosphere but the measurements now being undertaken with the aid of instruments projected beyond the atmosphere may be expected to add greatly to our knowledge.

The quantity of energy reaching the outer atmosphere depends on the distance of the earth from the sun and the condition of the sun. The variation of intensity due to the eccentricity of the earth's orbit may amount to a few per cent of the whole, and it is believed that some variation is introduced as a result of sunspot activity. These variations are small compared with the wide variations due to losses occurring in the atmosphere. Systematic measurements of solar intensity are made at a few stations in various parts of the world; such information is extremely valuable and one would wish for a much larger coverage.

Radiant energy reaches a man by several routes when exposed outdoors. Some reaches him directly from the sun, some is reflected from the surrounding terrain, and some is received from that scattered in the upper atmosphere. In addition, energy of predominantly long wavelength is emitted from the terrain as a consequence of its exposure to the sun. Clearly the altitude of the sun, and the orientation of the man with respect to it, affect the amount of energy reaching him.

In order to simplify the problem and to provide a baseline from which to work, it is assumed that the man and his clothing are ' black ', that is, that all the incident radiation is completely absorbed. This assumption is probably close to the truth for long wavelength energy, less true for the near infra-red, and obviously untrue for visible radiation, but results based upon it should provide values for the upper limit of exposure in all circumstances.

One way to measure the total incident energy is to devise an apparatus which is a reasonable geometrical representation of a man and capable of integrating all the energy falling upon it. The problems here are: What is a reasonable representation of a man from this point of view, and what man do we take as being representative of all men ?

In order to attempt to find part of the answer to these questions, and also to establish a method of finding the direct solar load, a photographic method has been devised to measure the area of a man projected in a direction normal to the sun's rays. An automatic camera and a flash unit are mounted on a platform which may be moved in a vertical quadrant of 15 ft radius. The subject stands on a turntable below the centre of the quadrant. The operator makes exposures at known angles of altitude between 0 and 90 deg. The turntable is then rotated and the exposures repeated; this process is continued until the subject is rotated through 180 deg. In this way a series of photographs are obtained which correspond to view points situated on the surface of the hemisphere surrounding the subject. The projected areas are found by means of a planimeter from images of the negatives. Measurements so far have been confined to the erect figure clad in the minimum of clothing; some 50 subjects, both men and women, have been photographed. It is hoped to extend the work to include clothed subjects and to other postures.

In order to find the direct component of radiation we need to know the solar intensity as well as the projected area; the product gives the required result.

If, with the aid of the data obtained in the way outlined an integrating apparatus can be devised, it should prove possible to measure in the field, the total radiant energy incident on a man. If in addition the solar intensity is determined with the aid of some form of radiometer, the direct component may be computed. The sum of the remaining factors may clearly be found by difference. It may be possible to separate these still further but the problems are considerable.

The development of a method of measuring the total radiation incident on the man should provide a means enabling us to begin to answer the question posed at the beginning of this talk. A complete evaluation of the problem requires a full understanding of the nature of the body as a radiator, in addition to more information concerning the variation in the spectral composition of the solar energy reaching the earth.

(5) " Visual Comfort and Glare ", by R. G. Hopkinson.

Glare can impede vision, by reducing the contrast between an object and its surroundings. In most practical lighting situations in buildings, however, the chief result of glare is not impairment of vision, but the sensation of discomfort which results when some parts of the visual field are much brighter than neighbouring areas.

This sensation of discomfort is of undecided origin at present, although various hypotheses have been put forward. Its study has been empirical, and a great deal is known about the physical conditions which cause it. It is now possible to design lighting in the sure knowledge that glare will not be present, or even, to a reasonable degree of certainty, to predict the degree of discomfort which will arise in any given situation. This is all the engineer needs to know, but the research worker will naturally wish to search deeper for the physiological causes of glare discomfort.

The study of glare has made greatest progress through the use of a technique called the Multiple Criterion Technique, a variant on a well-known psychological procedure. The subject is given control of one of the physical variables which has been shown experimentally to affect glare sensation—for example, the brightness or the superficial area of the glaring light source. When exercising this control he finds that he can change the magnitude of glare sensation at will, and quickly recognizes a series of levels of sensation which have been described to him precisely in words. This series is so chosen that the judgment of one level refines the judgment for the next, and so on. It is an improvement on straight-forward scaling methods because it makes use of the feed-back of information gained in one judgment to assist in the next. By this means, subjective judgments of surprising repetitive accuracy have been made by a very large number of subjects.

The experimental data has been expressed in formulae relating the most important physical variables, which are :

1. the brightness (luminance) of the glaring source (B_s);
2. the apparent superficial area of the source as seen by the subject (Q);
3. the luminance of the general surroundings (B_b);
4. the position of the source in the field of view;
5. the luminance of the immediate background to the source;
6. the shape of the source;
7. the luminance distribution over the visible surface of the source.

The basic formula linking the most important of these variables is as follows :

$$G = \sum \frac{B_s^{1\cdot6}\, Q^{0\cdot8}}{B_b^{1\cdot0}}$$

where G is the ' Glare Constant ' which expresses the magnitude of the glare discomfort. Where B_s and B_b are expressed in foot lamberts and Q in steradians a value of $G = 150$ signifies that 85 per cent of the normal population would experience glare equal to, or less than, a magnitude conveyed by the words ' just uncomfortable ' when explained precisely in the context of the experiment. In a practical situation, the interpretation would be affected by other factors—glare which is uncomfortable in a school might be acceptable in a serve-yourself store.

The causes of discomfort from glare are believed to be cortical in origin, whereas the impairment of vision by glare is at least in major part due to the optical scattering of light in the optic media. The degree of glare can be changed at will by changing the distribution of the light pattern which falls on the retina. When severe glare is being experienced, the pupil can be seen to undergo very slow expansion and contraction cycles. While some attempts have been made to use the absolute diameter of the pupil as an objective measure of glare discomfort these have been shown to be valueless. They have arisen because it has been demonstrated that subjects with their pupils under atropine experience a much lowered threshold of glare. Other physical symptoms of glare are contraction of the frontalis muscles, and lachrymation in extreme cases. None of these symptoms affords an objective method of estimating the precise degree of glare in any way comparable in accuracy or repeatability with subjective judgments by the multiple criterion technique.

Visual discomfort can be caused without a sensation of glare being present. Bright light distracts the attention even though it may not be glaring. The continuous effort of directing the attention on to the work, and away from a distracting bright (or highly coloured) area is a source of discomfort. The most comfortable conditions (not necessarily the most pleasant) arise when the work, or object of chief regard, bears to the immediate sourroundings the same ratio in luminance as these immediate surroundings bear to the general surroundings. This ratio is more critical the higher the general level of brightness. In a drawing

office it needs to be of the order of 10 : 3 : 1. This distribution of light in the visual field may be brought about by suitable distribution of illumination, or by choice of reflectances, or both together.

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Demonstrations :

(1) C. R. Underwood and E. J. Ward :
" A Remote Reading Vane Anemometer."
" Photographic Equipment used to Determine the area of the Body projected in a Direction Normal to the Sun's Rays."

(2) H. S. Wolff :
" A New Method for the Measurement and Recording of Integrated Skin Temperature in the Field."

(3) F. A. Chrenko :
" Methods of Measuring Air Speed."
" Apparatus used in obtaining the Threshold Intensity of Thermal Radiation necessary to evoke a Sensation of Warmth on the Forehead."

(4) B. E. Tredre :
" The Measurement of Mean Radiant Temperature."

(5) Mobile Laboratory.

Preliminary notice is given of the 11th Annual Conference to be held in St. John's College, Cambridge from 28 March to 31 March 1960 on the subject of Ergonomics Research.

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